DOCUMENT RESUME

ED 048 073 24 SO 000 820

AUTHOR Dunham, Richard M.
TITLE Conference on the Family as a Unit of Study in Social Problems. Volume II. Final Report.
INSTITUTION Florida State Univ., Tallahassee. Inst. of Human Development.
SPONS AGENCY Office of Education (DHEW), Washington, D.C.
BUREAU NO BR-9-0160
PUB DATE 1 Nov 70
CONTRACT OEC-0-9-190160-2332(010)
NOTE 101p.

EDRS PRICE EDRS Price MF-$0.65 HC-$6.58
DESCRIPTORS Conference Reports, Developmental Programs, *Family (Sociological Unit), Family Influence, *Family Programs, Human Services, Individual Development, Interdisciplinary Approach, *Preschool Programs, *Research Reviews (Publications), Social Development, Socialization, *Social Planning, Social Problems, Speeches

AESTRACT

This volume contains the papers presented at the conference in edited versions along with certain reference materials. As they influenced the proceedings of the task groups, these papers are reflected in the contents of Volume I (SO 000 819). As a group they cffer many useful ideas concerning the theory and methodology of family intervention and the strategic considerations related to them. The salient tcpics were: 1) Project Know How, an anti-poverty intervention program of social services directed at the mental development cf young children, and the stabilization and development of familial processes related to the socialization of the child; 2) a review of the methodological problems associated with preschool or early childhood intervention research; 3) public policy and the funding limitations on proposed national preschool intervention programs; 4) review of research involving interaction process analysis techniques; 5) program goals and operational definitions of the family; 6) the implications of the culture of poverty concept for family research; and, 7) the use of participant group methods (T-groups, sensitivity groups, human relations labs) with culturally disadvantaged families. (SBE)

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INSTITUTE OF HUMAN DEVELOPMENT

FINAL REPORT

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Richard M. Dunham

FLORIDA STATE UNIVERSITY

Tallahassee

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Volume II of II Volumes

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HEALTH, EDUCATION, AND WELFARE

Office of Education
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Conference on the Family as a Unit of Study in Social Problems

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Institute of Human Development
Florida State University
Tallahassee, Florida 32306

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U.S. DEPARTMENT OF
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FOREWORD

The Final Report of the Family Conference is divided into two volumes. Volume I contains the main digest of the conference discussion, together with additional remarks and perspectives intended to bring out the relevance of the conference for policy formation.

Volume II contains the speeches presented at the conference. Each speaker was allowed to edit his manuscript before publication. Dr. Oscar Ornati requested he be relieved of the responsibility of submitting a written manuscript due to illness but is treated in Volume I, of course. Dr. Paul Wohford's paper was not orally presented at the conference but was prepared specifically for the conference and distributed to the conferees; therefore, we have included it among the speeches.

My opening remarks will help the reader to understand the initial goals and orientation of the conference.

"The present conference is sponsored by the Bureau of Research, Office of Education. It occurs here because it was felt that the kind of whole-family intervention effort that we have in Project Know How made our campus a suitable location. The original formulation of the conference was achieved in an in-house position paper by David Bushnell, who is Chief of the Adult Education Section of the Bureau of Research.

"Behind the planning of this conference lies what I take to be a very general concensus: that it is time to seriously consider using whole-family intervention approaches as basic to the formulation of policies and programs designed to eliminate our major national social problems. The elements are not at hand to make a period of family-centered social planning successful. The elements are at hand in the same sense, perhaps, that the conceptual and methodological elements were at hand before the Manhattan Project was carried out. The fact that one can see the possibility of such planning, however, does not mean that there aren't a great many steps to be taken in actually developing the necessary technology, facilities, and so on.

"The objective of the conference, then, will be to foster an anticipated historical trend, one which I think will occur in any case, but may occur better and sooner by virtue of what we accomplish here we hope to contribute to the initiation of a wave of review of scientific methodology and theory, of agency programs and policies with regard to the family.

"I want to emphasize that it is not our goal to have a deep examination of some limited range of issues. With

conferees of this type it is possible to spend large proportions of conference time on methodologically or conceptually important but narrow issues, and to fail to cover in sufficient breadth the possibilities for actual action by other groups which should follow as a consequence of this meeting. I am calling for production of as great a range of views, range of concrete suggestions, as possible, even, if necessary, at the expense of depth of consideration.

"This means, then, that our meeting will support an advocacy, more, perhaps, than an evaluation of feasibility. There are agency personnel, members of the scientific community, committees of the Congress in need now of concrete suggestions on directions in which to move; and able and, necessarily in their roles, willing to do the critical thinking that would follow on the receipt of any one recommendation. So that, in the interest of productivity, some tolerance of the loose ends, the poorly worked out consequences of some recommendations, is acceptable.

"We are very much in the forefront of a change that is occurring, crystalizing very rapidly in the zeitgeist, a change toward emphasis on the family in our programs. It is not only the common sense of the professional community at large, as I have seen it, but it is the common sense of the community at large, and it is consistent with the common sense of the political leadership, both Democratic and Republican. It follows that one of the unique things about what we are going to try to accomplish here is that it is one of the few kinds of exercises that would have as much support from diverse elements in the community as this one will have. We can expect to have the support of the old and the young, the unpoliticized and the activist, of blacks and whites, of rural and urban people, of the community at large and the educated elements of the community. Such a program, if it is picked up by political leadership, can serve to reduce what has been referred to as the divisiveness that exists in the country as a whole. There are things that come to mind that have occurred in the Federal scene that support this view. The parent-and-child center movement of the OEO and the OCD is certainly one. (To mention only one other,) I will cite the work of Dr. Bobbitt, and the Joint Commission on Mental Health of Children and Youth, of which he has been Executive Secretary. He sent me the task force papers and recommendations of the Joint Commission have reached overall conclusions that is highly consistent with what we are trying to do here. It is to be expected, I think, that some of the details that come out of this conference will serve to support the recommendations of the Joint Commission, in very practical ways. Dr. Bobbitt reports that the Joint Commission is expected to recommend to the President the establishment of a high level family-and-child council. Such a council might work with the budget bureau for program planning and program criteria development.

The function here will be an advocacy. The Commission may also recommend that local family-and-child councils be established, to enter into planning with local agencies, with the end in view of gaining organizational structures and technology better suited to the support of the family and the development of the children. Their recommendation stems from the conculsion that problems of mental health must be handled preventively because there is not the possibility of mustering sufficient resources to handle them by correction. It is concluded that the support must be directed to the developmental process and its context in the family. I suppose an engineer might say that the Commission may propose that we work out effective quality control procedures with regard to the developmental process procedures that would operate for the most part through the family.

"Such a viewpoint is based on the observation that the literature of the social and psychological study of the development of personal characteristics increasingly shows specific family processes to the major determinants. This is true of those characteristics, such as ignorance, occupational incompetence, mental retardation, crime and delinquency, and mental illness, which constitute the personal aspect of our most serious national problems. It is also true of those qualities, such as intelligence, creativity, and good citizenship, which are our major national assets. Whether we focus on the developmental deficiencies and distortions of individuals or on their best adaptive capacities, those most satisfying to the individual or useful to the community, we find that the central contribution of the structure of the family, as judged by the scientific literature, is, to the greatest extent, neglected in our national planning."

In order to free up the time of the conferees, hosts were furnished. Most of these were graduate students in the social science departments and personnel from Project Know How. The hosts were "on-duty" for the duration of the conference. Their duties were diversified: taking charge of travel reservations; lost luggage; running errands; providing transportation to and from meetings; rounding up secretarial help; entertaining their respective participants; and acting as recorders during the task group meetings. These hosts were: Betty Bailey, Sally Carey, Bob Colston, Paul Croll, Eleanor Elfner, Mike Griffey, Lee Hall, Dot Hansen, Martha Holmes, Marilyn Leflurch, JoAnn Long, Melissa McDonald, Paul Mills, Jerome Novey, Don Shontz, Sylvia Smith, Virginia Stedman, Velma Williams.

During the course of the many months preceding and following the conference a large staff of people were busy writing the preliminary proposal, literature review,

editing video and audio tapes, giving counsel. To those who gave administrative, editorial and clerical support, we wish to extend our appreciation: Joy Byrne, Pat Catledge Agnes Chrietzburg, Darwin Dorr, Kay Floyd, Ann Freeman, Martha Futch, Georgia Guy, Cindy Harrington, Marjorie Harrington, Kay Hauser, Jennifer House, Shirley Meckley, Linda O'Neill, Becky Purvis, Mary Quinn, Louise Solner and Richard Grosclose.

A special expression of gratitude is deserved by my wife Jill, who was patient and helpful despite the many stresses and demands associated with the conference and the preparation of this report.

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THE DESIGN OF PROJECT KNOW HOW

Richard Marshall Dunham

The report that I wish to present draws heavily on the documents produced in Project Know How. Our several grant requests and progress reports were used by our staff to record their reasoning, their efforts, and their results thoroughly enough that the documents may serve as operating manuals. Other projects have used them to develop program components resembling ours.

My report will concentrate on the rationale for each of the main parts of Project Know How. Special emphasis will be placed on the involvement of the whole family, most particularly the father.

Some historical and methodological perspective will help to place Project Know How in the succession of developments in early childhood intervention strategies that have occurred nationally since the beginning of Headstart. Our original proposal appeared on the desk of Mr. Sugarman at the time he had taken over much of the management responsibility of Headstart from Dr. Richmond in 1967. Headstart was still primarily a summer effort and had not yet established its ascendance among the various competing anti-poverty strategies. The Parent-and-Child Center was being discussed in a narrow circle of specialists, but was not generally recognized as a possible strategy, and was unknown to those of us who participated in the design of Project Know How. The concept was not mentioned to us by Headstart personnel during the negotiations for the funding of Project Know How.

The project design was an extension of a line of reasoning I first offered, unsuccessfully, in response to the need for a central strategy for an interdisciplinary anti-delinquency effort in Houston, Texas. It was developed at Florida State University with the advice and consultation of a number of colleagues and specialists on the campuses of the Florida State University (FSU) and of the Florida A & M University (FAMU). The greatest direct support came from Dr. Ralph Witherspoon, Director of the interdisciplinary Institute of Human Development at FSU, and from Dr. Nancy Douglas, a colleague in the Institute and a member of the early childhood faculty. A great deal of constructive support and

intelligent criticism was also offered by Linda O'Neall, a sociology student who was my graduate assistant during the preparation of the proposal and who became a central senior staff member of the project. Thus, the project was both conceived and elaborated in interdisciplinary settings. I would doubt if it could have occurred in an orthodox atmosphere, such as that of an aspiring academic department. The interdisciplinary feature became one of the project's hallmarks, at times a blessing and at others a burden. After funding, the problems of interdisciplinary collaboration pose the greatest threat to the success, to the existence of such an effort. They remain, in the Parent-and-Child Centers, for example, a bedrock problem that has received insufficient direct attention. They offer fertile ground for research, and the promise of great reward if solved.

Project Know How was also distinguished by its status as an experiment. By contrast with the physical sciences, the behavioral and social sciences, and their various technologies, such as education, are overly passive, descriptive, and inductive. They are even ambivalent about their readiness to expend resources at the rate required for major experimentation. However, ready or not, it is now clear that we must offer more than descriptive studies, and Project Know How was a direct, intensive, experimental involvement.

The project was an experiment in several ways. Most obviously, it was a technological experiment, a social experiment in the engineering sense of the term. We were trying to put together a prototype of a model, just as aeronautical engineers put together prototypes of models of aircraft, with the major criterion for judging the results being whether it will work as intended.

In a discussion of these matters with the project staff, I spoke, as follows:

"In the end, to win the poverty war, we must solve scientific and technological problems, economic and morale problems, and moral and political problems. A visible result will be the existence of some prototypes, each one adequate to cope with a class of problems characteristic of poverty.

"The prototypes will be expensive. They will be the survivors among a generation of competing prototypes, all based on more or less precise and more or less adequate formulations of the poverty problem.

"Once constructed as prototypes they will be produced in quantity, like public schools or liberty ships, and the larger job will be done."

The project was also an experiment more or less within the laboratory tradition of science. We did not, for example,

admit all who were needy as service agencies frequently feel compelled to do. We were guided by admissions criteria that were intended to give us a sufficient number, over several planned replications with successive cohorts, of that population deemed most suitable for the type of test we were conducting. We carried out a number of experimental manipulations with our subjects, a sufficient number of manipulations on a sufficiently regularized basis to give us some hope of having produced an enduring, observable experimental effect. And we gathered data across the spectrum of that expected effect to see if we could objectively establish and perhaps even measure the effect. Finally, we established appropriate control groups and gathered as much data from them as our budget would allow in order to have an experimental comparison.

One element in the reasoning underlying the interpretation of Project Know How as an experiment seems to demand special mention. The experimental manipulations in Project Know How were several and complex, and the interrelationships among the manipulations that can be established as plausible are numerous. The experimental design does not permit the expected effects to be uniquely assigned to any one experimental manipulation. It has proven frustrating to the social scientist to be faced with the prospect of a range of experimental effects assignable only to a range of experimental manipulations. Our reasoning concerning this matter was that poverty and ignorance are not simple phenomena. We felt that the multiple breakdowns of the multi-problem family would necessitate several supports and corrections in socialization and other familial processes. We reasoned that the first task of social sciences should be to produce a working prototype, a model project that would reliably prevent the destructive effects of poverty on the socialization process and would reliably initiate a process of upward social mobility. We judged that simpler projects court failure from inadequate intervention and that their results, though sometimes lending themselves to straightforward interpretation are produced at a high risk of a) failure and b) misinterpretations due to the effects of uncontrolled variables omitted in the name of economy or experimental rigor.

We believed that the soundest long-range experimental strategy for anti-poverty intervention technology would be to carry out projects based on some estimated minimum sufficient manipulations to produce the desired effects reliably, and, then, to introduce the series of modifications and controls necessary to isolate the experimental effects of each component manipulation. One must realize that our reasoning was neither the typical child psychology research rationale, which seemed simplistic to us, nor the

service call for comprehensive intervention, uncritically providing whatever is "needed" under conditions so uncontrolled and unreplicable as to be without the possibility of assessment. We were endorsing a true experimentalism but on a scale suited to massiveness, the complexity, and the threat of the problem of poverty. We were underscoring the need for intervention with coherent subsets of manipulations, with adequate methodological safeguards to permit an effect to be detected, in order that it might then be followed up with more project variations designed to more precisely isolate the relations between project components and given project effects.

Again, let me draw on the address to the project staff, mentioned above, as follows:

"When a space-flight system, or any other cluster of components, is constructed in such a way that the success of the terminal function is dependent on the success of the components, then the reliability of the system is the product of the reliability of its parts. Thus, if a system has three components, each with 90% likelihood of functioning on a given trial, the likelihood that the terminal function will be completed is only 73%. With 10 components it would be only about 1/3.

"Suppose that some number of changes must be made in order to bring a child and a family from lower-lower class status to upper-middle in one generation. Must we not develop methods of accomplishing each component change reliably in order to hope to fulfill and secure performance at the new level? In our project and then nationally, in the PCC's, must our teachers not be able to reliably bring the child to normal intelligence? Must our supervising teachers, family service workers and several specialists not reliably succeed in establishing a strong, interdependent role division between lower class parents? Must the family service workers and occupational training personnel not regularly build in the husband a pattern of social skills, cognitive and occupational skills, and motivational characteristics sufficient to set him on a path of upward mobility.

"If all of the components of our system can be brought to work reliably, we can expect to regularly succeed."

In discussing our reasoning for intervening at several points simultaneously, we have raised an additional general issue, the question of amount of intervention. Besides the reasoning sketched above for multiple simultaneous experimental operations, there are two other frameworks within which I would like to locate the position of Project Know How. The first of these concerns the distinction between cross-sectional and longitudinal research, and the second concerns the proportion of the total mass of the direct experience of the subject that is influenced. These three

factors, taken together, relate to a broader issue which might be termed the social economics or social logistics of social learning. Under some such terms I would propose that we carry out a branch of environmental process analysis dealing directly in the absolute amounts and rates of delivery of the events assigned importance in social learning theory and that these analyses be extended beyond the study of the development of the individual to deal with the dynamics of the distribution of the events in the family and the classroom.

Project Know How was designed to stand high in both of the dimensions broached in the preceding paragraph. According to the original proposal we would have been deeply involved with the child and his family from the child's age one-to-two until the earliest elementary school years. In an era in which the dearth of longitudinal studies is deplored, ours was intended to be a true longitudinal study. Even as it finally worked out we had one cohort for over two years fully funded and beyond that for some of the children, using various local expediencies.

The last framework within which I wish to place Project Know How, insofar as social learning economics is concerned, deals with the proportion of the direct experience of the subject influenced by the intervention. Children were in the group care centers of the project four hours per day, less than often is the case with day care, but as much or more than is usual with experimental interventions. The child's mother was also present for that half day, and there is reason to believe she carried the lessons from the center back into her home. Moreover, in dramatic and, possibly tragic contrast to most agency efforts, and most special projects we involved the fathers in several ways. It seems possible, and many of our staff believe it is true, that we had built a model in which a very much greater than usual influence was exerted, through the nursery and the home on the direct experience of the child. The proportion of his immediate experience, in our view the raw material of personal and cognitive development, which was potentially altered in an intended direction was relatively large.

Just how we began to operationalize the ideas concerning social learning economics deserves to be described in brief. We proceeded to develop a General Behavioral Observation System (GBOS) and, within that system a counting and timing device we called the Behavioral Electronic Monitoring Unit. The GBOS was designed on the assumption that the developmentally significant qualities of a child, including those that are to be influenced by the educational process, have observable forms, and can be counted and timed. We sought to avoid dependence on ratings, test instruments, and other

measures that depend on longer inferential chains to support inferences about target behaviors.

The GBOS is capable of establishing the base rates and the determining conditions associated with any behavior, provided the frequency, duration, and occasion of observation is appropriate. Economy is introduced by the use of our time sampling plan, which takes brief observations randomly from predetermined 1) diurnal time state and 2) physical locations of 3) specified behaviors of 4/2 person or combination of persons. The brevity of the observation is established by pilot work as the minimum duration of observation that will yield stable counts of the frequencies and durations of the events being observed. Reliability and economy also govern the number of observations required in a given study.

As we used it, the GBOS focused on the occurrence of social reinforcers, not only because of their importance in learning theory, but because they relate directly to the literature on cognitive teaching style and disciplinary style. We found the experiences of Project Know How to be an overwhelmingly positive experience for the child.

In overview, the intended uses of the GBOS were the following:

1) To identify behaviors that
   a) are highly face valid.
   b) are observable with great reliability.
   c) occur with durations and frequencies researchable by our methods.
   d) are of established or high prospective construct validity.
2) To create a method of capturing unbiased samples of a subject's behavior.
3) To identify the environmental supports of a given behavior using the terms of social learning theory.
4) To characterize individuals in normative comparisons.
5) To achieve objective comparison of any two settings, such as our two Child Development Research Centers with each other or with those of another project elsewhere.
6) To achieve objective comparisons of conditions at two times (e.g., before and after treatment, in a single setting).
7) To determine the extent to which a program description, or other form of curriculum is, in fact, implemented, or is distinguishable behaviorally from one that is offered as very different.

Within the GBOS we depended on data-collection and data-reduction procedures of our own design. Our electronics

technician, Frank Schrama, ingeniously worked out and constructed circuitry to link counters and timers in tiers on a mobile rack. The resulting device, our Behavior Electronic Monitoring Unit (BEMU) became an object of interest and affection around the project, almost a mascot.

The BEMU fundamentally, is a simple device for counting and timing events, non-events (e.g., latencies), and contingencies of events in an observational study. It converts 110 volt AC electricity to 28 volt DC to drive its components. Patch panels permit contingencies of some complexity to be detected. Inputs are accepted from stimulus systems or from keys operated by observers. Observers, of course, may work with live behavior or video and audio recordings.

We think of the GBOS and the BEMU as operational contributions to the development of a social learning economics, although their use may be readily generalized. They are a small part of our research effort, but one that has proven to be very satisfactory. We expect to find other projects and laboratories taking an interest in this aspect of our work.

It is almost an aside that I take time to comment on Project Know How as a project which was racially integrated in a deliberate manner, and with excellent consequences. Our early decision to balance the number of black and white participants and staff might have been based on 1) a desire to compare intellectual response to treatment across race, 2) a desire to study, in sociological and social psychological terms, the process of racial integration and ethnic assimilation, or 3) an ideological commitment to the construction of a democratic atmosphere in which the socialization of children could proceed without the bias and brutalization that characterizes socialization in settings which are segregated or separatist. It was the latter, although the second became important as the project progressed.

The first possibility offers itself as a dangerously misleading question in science. It has approximately the same status as a scientific question as the comparable question about differences between the sexes, and if it occurs in a politically sensitive atmosphere, lends itself to invidious interpretations and to the acceptance of restrictive policies and legislation. Women's suffrage had to overcome the suspicion that women were unintelligent for genetic reasons, and that they were not adapted to act as citizens who must make responsible judgments for the society. Psychologists faced a considerable methodological and theoretical problem in producing measures of general intelligence that did not penalize women. The matter was finally solved theoretically by postulating that there are no genetically

determined differences in intelligence between men and women, and relegating any observed differences to the status of sub-culture effects. The matter was solved methodologically by choosing items and subscales for tests of general intelligence in such a way that neither men nor women would be penalized, on the average. In Project Know How we began with the postulate that no genetically determined differences in general intelligence associated with race exist, and never found it necessary to alter the postulate.

Operationally, we spelled out the postulate by assigning equal numbers of blacks and whites to each center, by insuring that work assignments of the mothers did not discriminate by race, by maintaining a personnel policy under which no position in the project was racially typed, and by discontinuing support of any project activity which segregated itself. Thus it occurred that, from the first days of the project, one could not tell who had authority, prestige, or acceptance, by his skin color. As one might expect, the policy attracted both favorable and unfavorable attention. Among the favorable forms of attention was the effect on recruitment of staff. Many fine staff members sought employment with Project Know How because it offered an opportunity to express their own personal commitment to the ideal of constructing a democratic society. In many cases their work reflected an uncommon dedication of energy, for the same reason.

Commitment to a democratic society was not a conspicuous motive among the participants. I think it was important to the black participants, and to a proportion of the whites, but it was handled by them on a very low key. The importance of improving the chances for health, success and happiness for one's infant child and the incentive money were the determinants of enrollment for the participating families. They overrode racial prejudice and other sources of reluctance in a way that seemed clear and final.

As our work progressed, however, we became more interested in the process of racial integration and ethnic assimilation. We found that our design based on principle was remarkably consistent with the findings in the social psychology of racial integration. An argument may be made somewhat as follows: As social distance increases, communication of unmatched attitudes is sustained, but communication of matched attitudes is hindered. Social distance may take the form of physical distance or of difference in social status. Matched attitudes serve as positive reinforcers, strengthening the communication tendencies and drawing people closer together in feeling. Unmatched attitudes serve as negative reinforcers, strengthening avoidance and rejection tendencies. Under these conditions, the Jim Crow variety of segregation obviously contributes to growing

alienation, black separatism invites the resurrection of Jim Crow, and simple physical integration risks failure due to residual status differences imported into the physically integrated circumstances. The best social engineering for a democratic institution would be one which provides for both physical integration and equivalence of status across race. Our search for an operational mode of expressing a commitment to principle had led us to design such a setting. I suppose one might say that ours was a successful hopothetico-deductive test of the principles involved, even though not originally intended as such.

Finally, I would like to observe that Project Know How was, by design a whole family effort. It was intended to take advantage of the strengths of the family, and to avoid dividing and damaging the family. No feature of the model so clearly distinguished it from other efforts. Its consequences are so important in principle and are so pervasive in our impressions of the project, that it will be discussed directly and by implication throughout this and other writing on Project Know How.

I will turn now to a description of the major components of the project, some of the reasoning under which they were developed and some elements of the experimental design through which they operated.

We thought of Project Know How as being based on two broad hypotheses, or, to be more accurate, two specific points of intervention. The first of these was an intervention in the mental development of the young child. The second was an intervention in the family circle intended to stabilize and strengthen the familial processes related to the socialization of the child.

These two points of intervention were approached through six major program components, the first three of which will be described below. They were the Children's Program, the Mother's Program, the Father's Program, the Research Program, the Health Program, and the Administrative Division.

The Children's Program was centered in a group-care effort carried out at two sites, each enrolling fifteen children, half of whom were white and half of whom were black. The children were present for four hours each morning, five days a week, for the full year. Most of them required transportation in project vehicles to reach the sites and return home each day. The daily Program resembled that of a good nursery. It was closely guided by the state-of-the-art of early childhood education. There was broad concern for the child's development. Health, nutrition, physical growth, motor development, social development, language development, and cognitive development all received attention.

The children were enrolled between their first and second birthdays. We chose the second year of the child's life to begin the intervention on the basis of an analysis of both practical and theoretical considerations. On the practical side were issues of the health and safety of the children in a group care setting and a concern not to invest in an intervention until the latest time that we could be confident that social class-related deficits in development could still be prevented or corrected. On the theoretical side we were influenced by evidence that social class-related deficits emerge between about eighteen and thirty-six months of age. Our review of the literature on the impact of social class standing on child development also left a vivid impression that what emerges in that interval is a stable configuration of factors, including language development, scholastic aptitude, and measured intelligence, all of them strongly and positively related to social class of origin. We felt that the evidence that a child attains a relative stability of rate of development with some predictable ultimate asymptote for these factors was sufficient grounds for entering the child's life at the apparent beginning of the class-related differentiation.

Our reasoning about the best age of intervention obviously would not apply to infants with serious organic complications, such as might be caused by disease, birth trauma, or some forms of malnutrition. Our selection criteria provided for such children to be eliminated in the screening process. With only a small number of exceptions, we believe our selection was effective in this regard. We recognize, however, that an attempt to generalize the application of the model to special populations would require reconsideration of the age of intervention, at least for families with high risk of organic complications.

As the Children's Program developed we found ourselves beginning to measure the amount and quality of adult attention given each child. Increasingly procedures were introduced which would insure that each child had not only exposure to a generally happy, healthful and stimulating atmosphere, but also to at least some specifiable minimum of highly individual, verbally and socially sophisticated adult attention. You see, we were beginning to anticipate the emergence of our concern for social learning economics, but our anticipation took the form of a concern not only to influence the rate of change in the average developmental level of the children, but also the rate of change in the variability among them. Specifically, we wanted the group to show good average gains, but we wanted to minimize the degree to which any children lagged behind the group as a whole. One recognizes that such a policy has consequences for the social structure of the class and the experimental design, as well as for the welfare

of those children who may begin with some handicap such as lack of language experience, shyness, or belligerence.

We also found our Children's Program to be accelerating because of the increasing educational effectiveness of the mothers, which brings us to the description of the Mother's Program.

The Mother's Program was also based primarily in the child development centers. The mothers were hired at a minimum wage, half time to work as aides, we called them Assisting Mothers, in all of the areas in which things must be done to make the project successful. Their duties, while real and necessary, included much in-service training, and opportunities beyond their working hours for additional adult education. The curriculum to which the mothers were exposed in their work and study was drawn from home economics, early childhood education, adult education, and health education.

As with the Children's Program, the design of the Mother's Program was based on both practical and theoretical considerations. From the practical viewpoint, we felt that the children were at an age at which the mother's presence would solve many transitional adjustment problems and that program costs would be held down without risking damage to the child's development by reducing the number of full professionals to be hired. On the theoretical side, there were a variety of factors. We did not want to force separation of the mother and child during infancy. We wanted a chance to influence the cognitive teaching style and the disciplinary style of the mother. We wanted to be able to strengthen and stabilize the mother's role participation in the family as a whole. Hence, we wanted a chance to bring her various role functions under supervision and under the influence of role models. The group care setting, with real duties assigned proved to be a highly effective way to accomplish this without resorting to oppressive, intrusive, or paternalistic techniques.

We set no concrete goals for the mother's educational or occupational attainment. We envisioned being able to certify the mothers for employment as highly qualified early childhood education aides, but we did not have to face a problem of conventional occupational training and placement. We also developed a policy by which we could avoid dependence on any techniques which implied or required that the mother be literate. However, the mothers themselves very early developed adult educational aspirations, perhaps influenced by the high aspirations they found themselves developing for their children.

As mentioned above, the mothers came to take an increasingly active part in the specialized educational activities used with their children. We recognized and encouraged this shift to a technically oriented role with a new title, Consulting Mother. Under this title, the mother was paid at a higher rate, and for services to children, rather than time on the job in the center. Mothers were phased into this specialized activity as they showed interest, using a technique based on social learning theory. The mother was introduced into a situation in which the child was learning relational concepts and other things in a relatively free exploration of some special materials in cooperation with a professional tutor. As the observing mother involved herself spontaneously with the child and the materials, having seen the modeling provided by the tutor, the tutor withdrew from interaction with the child and encouraged, positively reinforced, if you will, the mother's involvement. The result was a very happy exchange, characterized by much joy and spontaneity on the part of the mother, now acting as tutor, and the child. While the tutor could answer specific procedural questions, could listen non-directively to any expressions of concern by the mother, and could provide the mother with a specially developed manual, she could not engage in any negative criticism or direct correction. Beyond representing a fulfillment for us in our efforts to improve cognitive teaching style, the method led us to begin to consider use of the mothers in other technical areas, such as observation and other basic data-oriented operations.

The Father's Program of Project Know How may be the single feature that sets it apart from other interventions most dramatically. We know of no other program, not even among the Parent-and-Child Centers that has yet equalled our intensity or quality of involvement of fathers. Nevertheless, the Father's Program was the most poorly funded part of the whole effort, and was the portion that developed most slowly for us.

Our reasoning in insisting on a Father's Program rested on several key considerations. Of course, we wanted to strengthen the father's role-playing in the home. We focused on his roles as breadwinner and head-of-the-household, just as we had focused on the mother's role as homemaker. We also wanted to influence some of the same dimensions of child care as for the mother. In addition, we wanted to reduce the likelihood of a marital failure which would in turn drastically reduce the amount of adult support available to the child and the possibility of setting up a rate of upward social mobility by the family. We were aware that the literature on the natural history of the family indicates that the periods of greatest danger of marital failure are when children are very young and place great demands on the

parents. We knew that our target population already carried a high risk of marital failure, and that a shift from two submarginal incomes totaling one that is marginal or better with both spouses employed, to one submarginal income if the mother is forced into child care at home could prove devastating in many families. We recognized that employment was often more readily available to wives than husbands and that employment might pay more at the lowest social class levels for women than for men, thus inviting role inversion and intolerable stresses related to sex role typing. Whatever the future of our society may provide in these matters as alternatives, at the lower-lower class level the importance of achieving stable, socially and economically effective role relationships in order to meet the requirements of the child for socialization and the requirements of society for upward mobility is manifest. We are not indulging in a form of chauvinism, but attempting to estimate the soundest strategy for the family that aspires to improve its comfort and standing in society despite great handicaps. It may help to quote from our original grant proposal, as follows:

"We draw on the social science view of the family as a balanced and effective social and economic unit. According to this view, the middle-class advantages in education and income are a consequence of familial role stability, emphasis on personal ability and sound work habits. The models and continual attention typically provided the children of the middle-class transmit the strengths to the next generation. By contrast, in the lower-classes, special stresses impinge on normal familial role-playing and reduce the likelihood of its being sustained satisfactorily. And, as stress-induced role deficits increase, the adequacy of socialization decreases, with the result that deficits are still more likely in the next generation.

"Effective stresses may include unemployment, underemployment, inadequate housing, and other economic factors. They act with particularly great perversity and severity on the father's dual role of breadwinner and head-of-the-household.

"The lower class father is likely to be suspect, at best, in his own home. Even when the family to some degree realizes the extent to which it is a victim of its circumstances, there will be the closely related realization that relative personal inadequacy is also involved. The husband is a disappointment to his wife; the father is an object of shame to his son and daughter. His family is painfully unable to operate with acceptable social freedom and mobility in the community; it loses its prestige and voice of citizenship in community affairs. The father, and the family, assume an attitude of humility which is engendered, not of pride and strength, but of failure and hopelessness.

"The lower class father, at worst, is rejected in his own home. As the father falters and fails under the mounting pressures the likelihood of his displaying unacceptable behaviors increases. He will 'be' irresponsible, drunken, unreliable, etc. Economic inadequacy converts, through its impact on the father's social role adequacy, into personal inadequacy. Family failure by separation, divorce, desertion, and stress-induced disease become common.

"These conditions are further aggravated when the community makes job opportunities available to the mother and not to the father. . . The mother's apparent economic advantage thrusts her into the role of breadwinner, and by implication, head-of-the-household. Her economic 'advantage', however, disrupts the normal pattern of role relationships, and reduces the family's economic potential. Her 'advantage', in fact, helps to condemn her to poverty.

"Such natural histories may take years. On the other hand, the process frequently is accelerated, sometimes causing familial failure before the relationship matures into a public and lawful marriage. A sequence of attempts to succeed, leading always to disheartening failure, leaves the too typical lower class family, truncated by the absence of a stable male role.

"With failure, the responsibility and the authority of the father's roles devolve to the mother. With successive failures, a distillation, and crystallization occurs, in which the reassignment of roles is practiced, habituated, and partially acculturated. An adjustment to the tragedy of failure of normal familial roles becomes a defining characteristic of the subculture of the impoverished. Matriarchy by default becomes a model of family life which is perpetuated in the socialization of children.

"Project Know How has chosen as its target population, those younger families which are still, at least nominally, intact. The Project proposes to intervene before typical stresses have destroyed the semblance and possibility of normal role practices."

Having employed the mother half time at a minimum wage in a position congruent with her homemaking responsibilities, we had made it possible for her to invest her remaining time in her family, rather than in submarginal employment. We then devised a series of supports for the father's roles. By far the most fundamental element of the Father's Program was the role of the Family Service Worker. The Family Service Worker was a male, middle-class role model who served as a liaison between the project and the family. He dealt to the greatest extent possible with fathers, but, when necessary with other members of the family, as well. His primary mission was to establish and maintain a deepening rapport with the father. He also served as a resource person, helping the father to develop his goals, to acquire benefits

from other agencies when needed, and to find additional employment or education. We relied on existing agencies for traditionally available help in education and occupational placement, but we conducted a special version of human relations training which we evolved over the length of the project. Human relations training in Project Know How had to hold the interest of low income fathers, dealing with their real problems, rather than merely sensitivity to others. It had to hang together with men who knew each other and would continue to. It had to be effective in sessions of a few hours duration on a weekly basis, rather than sessions that occupied much of the day for a few days. It had to be able to succeed with a membership composed of low income whites and blacks. Our ultimate success was achieved with a training content that did not highlight individual weaknesses and failures but invited the fathers to review and criticize the component functions of the project and offered them good information on the real problems they faced as a group, e.g., buying a car.

So much for the description of the components of the project. I would like to turn, now, to a recognition of two consequences of our familial focus as it interacted with the institutional structure of Project Know How and as it highlighted the contrast of the structure of Project Know How with our well-established front-line social service institutions, such as the public schools, prisons, or hospitals.

In the same staff address mentioned above, I raised the problems as follows:

"While we must build and understand our prototype in terms of its components and our minds require us to simplify such things into parts in order to grasp them, the basic phenomenon does possess an integrity of its own. That we apprehend that integrity is reflected in terms like 'poverty-ignorance cycle' or 'culture of poverty' or 'multi-problem family' or even 'lower-lower class standing', all implying effects on competence, on measured intelligence, on prestige, on vulnerability to disease, and so on. As James held experience-in-general to be, the phenomenon with which <u>we</u> are dealing is 'continuous and concatenated' and may be apprehended as a unitary molar phenomenon.

"To cope with a powerful configuration of effects associated with the term poverty, we have typically, almost universally relied on fractionalized approaches. As our minds have over-simplified the problem in this way or that, we have devised techniques or agencies with limited and inadequate powers. And with great regularity they have been drawn into the vast quicksand of the total problem or they have escaped into the service of the middle class. (In

either case they have typically failed to assemble acceptable evidence of effectiveness.)

"Let me make the same observation from a slightly different perspective. In Project Know How, we have become aware of the necessity to delegate authority and responsibility from any central administration into the operating sections. In doing so we verge on the creation of separate agencies. Perhaps we are protected from being divided and scattered by a relatively coherent basic formulation. Nevertheless, it is true that the simple magnitude of the administrative requirement acts as a centrifugal force on the distribution of the power of the project.

"Project Know How was designed in response to a call for comprehensive and innovative approaches to the problem. It grew, also, out of a local decision not to recommend a mere amalgam of off-the-shelf components, but to seek a set of principles under which standards of both (1) appropriate comprehensiveness and (2) coherency could be applied. In such an approach, comprehensiveness should be obtained by derivation. In such an approach, not all services are implied.

"An engineer or mathematician might say we were seeking an elegant solution. At the end, we hope our prototype will not only reflect elegance but parsimony. On the other hand, we do not believe it can appear as an oversimplification by comparison with other prototypes. It will be seen as comprehensive.

"It has been in the process of making our derivations, that we became so aware of the molarity of the problem and of the inadequacy of most existing prototypes. Sometimes, in the search for correct, coherent derivations, we have become aware of pressures or tendencies to elaborate the basic postulate set, to broaden the formulation. In their most concrete forms they arise as questions like the following: Should we have non-group-care conditions that are tutorial or involve cottage care? Should we have a straight income subsidy condition? Should we accept a sample in which the father is not in the home? And above all others, the always poignant, sometimes demanding call for increased services or for larger samples.

"But correct derivations are not automatically obvious, no one involved has access to full range of information to support decision-making - ie, theories, doctrines, and techniques from several disciplines, on one hand, and direct acquaintance with the phenomena as confronted on the line, on the other. To cope with the diversity of relevant information a genuinely interdisciplinary approach is needed."

Although Project Know How was designed "from the ground up" as an innovation in institutionalized forms of delivery of social services, it was not without serious basic design problems. Perhaps the most serious and basic problem seemed

to be a consequence of attempting to deal with the whole family. The attempt, we found, created a very great requirement for communication, coordination in decision-making between the family and the staff, and between various staff components. This requirement often was most apparent when it was not satisfactorily met. Then it would show up in social conflict, often passing for interdisciplinary conflict. Sometimes it would assume the form of staff paternalism, or parental withdrawal from full participation. The conflict tended to focus on some front line staff positions and in the supervisory positions to which they reported directly, and to be experienced emotionally as anxiety and anger. We came to feel that the staff positions most severely affected were subject to increased risk of disease, possibly of stress-induced disease. Whether we can expect to become able to operate simultaneously in relationships with several members of a family simultaneously, carryingout a range of operations with personnel from various professional backgrounds, without conflict and stress, is for us a hypothetical question. Building a prototype, as opposed to operating a model in which tested roles are being carried out by personnel trained to operate within the model may require a great deal more communication and decision-making. When expectations are crystallized more, a manageable level of requirement for communication and decision-making in regular operations may result. We question, however, whether the level can ever drop to that of the typical service agency of today, to a level determined by a requirement to deal only with a child of a given age, or a given class of social or physical pathology, or an accepted, if not broadly validated, set of methods characteristic of the profession which dominates a given agency. We offer the problem as one which must be subjected to empirical research. We should be able to determine how many people a nursery teacher must talk to, and how much, and for what coordination or decisions in a family centered operation and in a conventional, factionalized agency approach. We should be able to introduce improvement experimentally, once base rates have been discovered. At the least, we should be able to discover which roles carry impractical communication and decision-making requirements, as determined by actual comparison of time requirements for these with time available.

Our experience with the communication and decision-making load in relating to whole families in Project Know How led us into some speculations about the design of the institutional structure of conventional front-line social service agencies. We were struck, as we contrasted them with Project Know How, that their relatively simple structures might be related to their apparently less demanding roles and role interrelationships. A classroom teacher in a typical public school thinks of herself as teaching the

child. She may know that parental influences are important in determining the child's classroom performance, but her role is not so structured as to permit, nor even to qualify her to do anything about it. She may be aware that health problems affect the child, but, again, she is really not trained nor expected to do much about it. She sees emerging indications that the child will have mental illness or criminal tendencies, if she is able to see them at all, primarily as they make teaching more difficult. Her role, then, fractionates the family; she typically deals with only the child. It fractionates the child; she really doesn't have time or training to do much except teach in the classroom. It is only when the family characteristics or the "irrelevant" qualities of the child so seriously interfere with traditional classroom goals and methods that she cannot even seem to be teaching what is necessary, that she and the school becomes concerned with whole family and whole child considerations. On the other hand, if the teacher and the school can reject the broader concerns, they have the reward that roles are less demanding. The level of the communication and decision-making requirement, whether real or illusory remains within the range of practicability.

Analogous reasoning may be offered for other front-line service agencies, such as hospitals, police departments, and churches. One is impressed that all such agencies fractionate the family and the client, that all are oriented around a class of social or physical pathology, that all depend primarily on a given profession or group of professions together with whatever is good practice for the profession. The advanced literature concerning each agency may deplore the limitations and may call for breadth, but the breadth that we see achieved is not usually impressive. We are left with the feeling that the limitation is ground in some hard sociologically definable limiting conditions, and that the aspirations and exhortations are to no avail, until the limitations are discovered and dealt with. I am suggesting that it is no accident that institutional goals and structure are recognized as fractionalizing social problems, families, and individual clients. The limitations are there, in the as yet unresearched requirements for communication and decision-making. As an agency moves from a requirement to deal with only one kind of problem with only one kind of client by methods of only one discipline, to dealing with a range of kinds of problems, in a manner that supports primary group (familial) function, and allows simultaneously application of the methods of several disciplines, the actual number and cumulative time involved in telling important others what is to be done and gaining their approval may increase at a rate that accelerates so rapidly as to rapidly take the time and event requirement beyond the capacity of the system. If so, the relative simplicity of the institutional structures may

represent a limit to which they have evolved and beyond which they cannot move without increases in our scientific and technological grasp of the limiting conditions.

In Project Know How, we found that the total load in front-line staff meetings, small staff conferences, supervisory exchange, central resolution of policies, family consultation, and so on was very high. We felt the dilemma that we needed more such time but wanted less. We felt, individually, that we should just go and do our job to help the family, but the complexity always drew us back into the effort to communicate and make the necessary, innumerable decisions.

In conclusion, let me emphasize the two characteristics of Project Know How, by now obvious, that seem to me to be of the greatest significance. One is a positive advantage. It is that whole family involvement is inherently motivating to all of the family members. This feature produces benefits in easier recruitment, lower attrition, better cooperation in project procedures, and smoother introduction of new values into the socialization process. The other is negative. Briefly, it avoids competing with the functions of the family and avoids eroding the cohesiveness of the family through a mindless dedication to a single category of problems, an overly narrow target population, or the methods of a single group of professional disciplines.

METHODOLOGICAL ISSUES IN PRESCHOOL INTERVENTION STUDIES:

or, if the Hawthorn doesn't get you the Rosenthal will

Francis H. Palmer

Our nation's attitude toward preschool intervention programs seems to be pretty much that of my granddaddy's toward whiskey: "there are no bad whiskies; some are just better than others." Both Martin Deutsch (1967) and H. L. Miller (1967) have offered elegant statements recognizing that preschool training programs are uncritically assumed to be effective. Confidence in their effectiveness is often so elevated that early childhood intervention is accorded the status of a virtue.

Proceeding on these assumptions, large sums of money and enormous efforts are expended each year for preschool programs, most of which do not pretend to evaluate their results. The success of the preschool program is frequently a function more of the motivation of the person or persons involved and the level of their evangelical zeal than it is the creativeness with which the learning situation has been contrived. In terms of precise statements as to what has occurred to the child as a function of the treatment, nothing but biased observations by parents or program heads are usually possible.

To my knowledge the last man of my faith to publicly assail motherhood was Harry Harlow, in 1957, wherein he declared himself to be a better mother monkey than his mother monkeys were, and Harry eventually had to retract even that statement when his subsequent research returned motherhood to the nature of love. Where he failed I shall not tread. Thus this paper will deal only peripherally with motherhood, but more centrally with that assumption that all early childhood intervention is unassailable. What must first be shown is that a specific kind of intervention is beneficial and that its effects are durable.

It is the less frequently supported program which does attempt to assess its results in terms of the intellective, social or emotional change in the children concerned. These

studies are even more expensive if done dorrectly, and the responsibility on the investigator is considerably more than for the dedicated operator of a creative preschool program. He is responsible for more than creating an aura of beneficence about what he is doing; he must show clearly that his treatment made specific effects or be prepared to admit that they did not.

Today I am going to discuss those steps through which a researcher must go if he is to demonstrate the beneficial effects of a given preschool program and presumably to report his results where they become public information and open to validation. In discussing this, I am also going to point out the traps, snares, ambushes, and seductions inherent in each step by, in most cases, drawing upon my experience in Harlem where we have for three years been working with Negro, Puerto Rican, and white children age two to four and a half years. The study is supported by NICHHD and presumably will continue until the children have completed the first grade, so that the durability of the effects of our treatment may be assessed, as well as those specific dimensions of behavior which are influenced by our treatment.

I. SELECTION OF THE PROBLEM

The first problem an investigator faces when he has made that decision to move from individual experiments with small samples and precisely defined variables to intervention over time with a multivariable treatment is in fact to define the problem. This may appear simplistic, but I can assure you that it is at this step that most intervention programs ultimately flounder. Caught in his new humanitarian zeal, even the most cautious researcher tends to take on more than he can handle logistically and more than he can handle scientifically. Every logistical miscalculation eventually manifests itself in a compromise of what the study was originally designed to test. And every additional variable the investigator desires to include in the design of his treatment inevitably increases the complexity of his logistical problem. (Parenthetically I can say that this is the reason that logistical activities in a large program must be supervised by a scientist--only he can understand what a minor modification in logistics means for compromising research design.) For example, the larger the sample of children and mothers required to answer the question asked by the investigator and the larger the professional and supporting staff required to implement the study, the greater the chance will be that someone has an affective relationship, positive or negative, with someone else in one form or another and that relationship may effect the research.

Instructors, children, vehicle drivers, mothers, receptionists, and secretaries get sick, bored, angry, tardy, obstinate, and seductive, and any combination thereof will increase the logistical complexity of a study to the point that the mind boggles. If the logistics of an intervention problem will so dominate the day to day activities that it threatens emphasis on the research, the investigator should seriously question whether he wishes to begin. There simply is not time to treat this subject adequately in this paper, but the giver and receiver of funds for intervention studies should consider the logistical load of the study as seriously as any other dimension.

A second dimension which influences the selection of a problem is the cost of the intervention study. Characteristically the investigator bites off more than he can chew. He assumes greater accessibility of subjects than is warranted; he assumes plans on paper will be turned easily into action; and almost always he introduces more variables into his treatment than he can account for in the analysis of his results. Each of those mistakes increases the ultimate cost of an intervention study.

There was a time two or three years ago when it appeared that there would be adequate funding for large studies for as far as one could see ahead. Not surprisingly, many investigators initiated comprehensive studies of considerable cost, and settled for annual review of funding. As money became tight, most of those studies had to cut back with respect to some aspects of the problem they had chosen to study, and that cutback inevitably affected the research aspects of the program first. Whenever you are forced to compromise in an intervention study for whatever reason, it is the research that is hurt. Consequently, any time that a large intervention study is designed to be primarily concerned with research results, the investigator should not consider a problem for which he cannot obtain clear funding for the duration of the study. He can easily lose two or three years of his professional life if he does.

Granting agencies, on the other hand, should evaluate a study as to whether they are expecting research or demonstration profits, and if they expect research profits, they should fish or cut bait with respect to promising support for the duration. Do not expect research results from a study which has to compromise its design from lack of money.

But there is another type of cost consideration. What will the implications to the nation be for the study considered if the results were applied to large scale preschool populations? While the latter may be of less significance

to the researcher in selecting a problem than to the representative of the agency who will support the research, the investigator should be prepared to speak about this point and to show that he has considered it in the design of his study.

What then, are some of the parameters which become involved in the choice of a problem?

What specific treatment the investigator eventually decides upon is a function of a variety of factors, some of which are worth discussing here: the theoretical bias of the investigator; the age at which the treatment will begin; the saturation level of the treatment; the training environment; and the ratio and relationship between instructor and child. Each interacts with the other in decisions about the intervention or treatment, and all are related to the selection of a problem.

1. Theoretical Bias of the Investigator. Presumably the treatment will reflect a theory or rationale which the investigator embraces about children's development or learning. Ideally there is a body of literature which supports the use of the variables which comprise his treatment. He will have a bias, perhaps towards the emphasis of language development, or towards non-linguistic aspects of behavior such as experience with sights or sounds; or, his bias may be such that he prefers to involve a multitude of variables which hopefully will influence at once various aspects of the child's social, emotional, and intellective growth. His bias is important with respect to the kinds of statements he can make when he has finished the study. The more complex the treatment, the more variables involved in the stimulus part of the intervention, the less precise the statements he can make about his results. To increase the total enrichment in terms of the number of behaviors affected means to decrease the probability that one can specify which variables within the treatment brought about the effects, if indeed effects there were.

Some existing studies illustrate these different goals: Earl Schaffer is involved in a study primarily concerned with language development and the nature of his intervention reflects his bias. In the study we are doing in Harlem, we are concerned with the development of very simple concepts, such as: up-down, rough-smooth, warm-cold, which increase in complexity with the child's age and experience.

On the other hand, Dr. Dunham at Florida State has a much more all-embracing treatment, one aimed at the intellective, social, and emotional development of the child. The

same is true for the University of North Carolina study, where a large variety of behaviors are purportedly influenced as a function of a saturation treatment. This diversity is important as we evaluate early childhood education because in one sense one man's guess at this point is as good as another. However, it should be remembered that comparisons among studies are only possible where change as a result of intervention has been measured with similar evaluation procedures.

2. Age of Subjects. Critical to the selection of a specific treatment and related to it in rationale or theory is the age at which the treatment presumably would have its maximum effect. In the last few years we have seen studies initiated with younger and younger children, partly because the effects of interaction programs have been disappointing in terms of durability and partly for theoretical reasons. In our Harlem study, we began at age two because we felt strongly that the earlier we could train the child in fundamental concepts the more we would influence cognitive behavior and the more durable the results would be. Furthermore, the teaching of such concepts earlier would have been more difficult administratively. At age two, even if the child is not speaking, he can comprehend a great deal of language when spoken to. So age two in our case was a compromise between administrative feasibility on the one hand and our desire to intervene as early in the child's life as possible. While children vary widely, the age when they are exposed to the treatment is, on the average, a matter of real concern in the development of a treatment.

3. Saturation Level. The third consideration in the selection of treatment is the saturation level which the child must be exposed to in order to best respond to the treatment. By saturation level I simply mean how many hours per week the child is exposed to the intervention environment. Clearly, saturation level has great implications for a program involving two year olds. It means that a child can be exposed to a program requiring participation of two hours per week as compared to one which requires forty hours per week. The implications are great for the mother, for the logistics, and for the relationship with the theory or rationale the investigator defends. As with all of these considerations, we are fortunate to have existing diverse studies which represent the extremes of child-time involvement. For example, our Harlem study requires the child to be present in the laboratory two hours each week; the North Carolina study conceived by Halpert Robinson is the ultimate in saturation, barring placing the child in a krech, as he has them in the training environment practically from dawn to dusk. We need to know the different effects which result

from such minimal and maximal treatments, and providing that our measures are comparable and our populations well defined, such comparisons may be possible. Most likely we will ultimately find that with two hours per week some goals may be accomplished and with 40, others will be accomplished.

When saturation level is considered, however, it is probably mandatory that we consider individual differences among children. Children differ vastly at this age with respect to their language development, their conceptual development, the degree to which they can be separated from their mother without undue frustration, etc. Some day perhaps we will know enough about the characteristics of a child to determine whether one kind of treatment or another is best suited for the child. It is an area of research which has received little attention, but unless we are willing to assume that all children at a given age will benefit from the same intervention we had better look at this point too.

4. The Training Environment. The training environment deserves mention as a variable related to the rationale of a treatment although there is little time to discuss it thoroughly here. Suffice it to say that at least four possibilities are represented in studies which quickly come to mind: a laboratory setting as represented by the study in Harlem; a nursery setting as represented by the studies by Dunham and Robinson; a more formal school setting as represented by studies by Bereiter and Caldwell; and the home setting, where the instructor goes to the home, as represented by Schaeffer. Clearly it is a variable of significance. Logistical complexity is also closely associated with the definition of the training environment and should be attended in the selection of a treatment.

5. Instructor-Child Ratio and Relationship. Finally, both the instructor-child ratio and the relationship between instructor and the child varies from study to study. Each of course is a legitimate variable to be considered in defining a treatment.

Where the investigator is concerned with a broad enrichment program designed to influence the social, emotional, and intellective development of the child, there are likely to be a greater number of instructor-child combinations. Not only does the investigator have to decide how many children each instructor will deal with, but whether others may be present during the treatment whose relationship to the child differs from that of an instructor, i.e., the presence or absence of the mother.

At the other extreme, our Harlem study is concerned with a narrower aspect of the child's development, his learning of various concepts as outlined in a prepared curriculum. This decreases the number of possible instructor-child combinations, and the one we have selected is the simplest, a one-to-one relationship between instructor and child without interruptions by a third person. Thus, the focus of our study became the affective relationship developed between instructor and child.

II. SELECTION OF SUBJECTS

A second source of major concern in the designing or evaluating of intervention studies is the selection of subjects. Traditionally subjects are selected for convenience or accessibility rather than for the attributes most relevant to the problem under investigation. An illustration has come to my attention recently.

An intervention study purportedly covering race and socio-economic class reported its experimentally enriched lower class Negro subjects to be skyrocketing in IQ's and other demonstrable characteristics of intellective performance when compared with their controls. However, questions about the specific background of the lower class experimental group revealed that most were recruited from families of university janitors who understood the benefits of having their children the subject of additional educational input. The lower class Negro controls, on the other hand, were selected from the community at large, and because they were less accessible were placed in that control group, where they were involved in the study only for annual assessment. Furthermore, the university janitors had a long history of agility when it came to getting their children into college, implying to some at least, that the experimental parents were of unusual intelligence and motivation as compared to the controls. With a comparison between experimental and control groups selected in that manner, no possible conclusion could be made about the specific effects of the treatment to which the children were exposed.

Proper selection of subjects is time consuming and expensive. Let me describe a subject selection system used by our Institute which suggests the effort and cost involved. Our study required 310 Negro boys who would be two years of age on October 1, 1966, and who fitted into a previously determined distribution of socio-economic status ranging from children of upper middle class to lower class. Below, the stages of the selection process are outlined:

1. Birth Record Search. From New York City's Bureau of Vital Statistics, 1461 birth records of male Negroes born between August 1 and December 31, 1964, were copied. Excluded were children under five pounds at birth or who had experienced unusual complications during or preceeding the birth, twins, children whose mothers had a history of narcotics or syphilis, and those born of mother younger than 15 or older than 45 years of age. All records copied were of children both of whose parents were Negro and who resided north of 80th Street in Manhattan at the time of birth.

Preliminary classification of SES was made from information about the father's occupation on the birth records, using the Hollingshead-Redlich Two Factor Index of Social Position. Occupation ratings of 1-5 were considered middle class and 6 and 7 as lower class. Where father's occupation was not listed, initial classification was designated "unknown." These classifications were maintained until after at least one parent had been interviewed in the home and current data on the education and occupation of the head of household had been obtained. Families falling into Hollingshead-Redlich categories I-IV were rated as middle class; those falling into category V, as lower class.

The pool of subjects was then drawn upon and subjected to the procedures listed below as different age groups were needed. Thus, interviews with mothers of children to be tested at 2-0 occurred within two months after the birth record search, and mothers whose children were to be tested at 3-0 were interviewed fourteen months after that search.

2. Post Office Confirmation of Address. As age groups were required for testing, all relevant names and addresses were taken from the subject pool and sent to the Post Office. These were checked to see if the family still lived at the same residence or had moved to another location in the same area of Manhattan and had left a forwarding address.

3. Interviews Attempted. As names were drawn from the pool, letters were sent to the parents informing them of the study and asking them to expect a visit from a member of the Institute staff in the near future. Interviewers called at the address up to six times before assuming that the family no longer lived at that address and had not notified the Post Office. When it was estimated that a sufficient number of interviews had occurred to satisfy the needed sample for the larger intervention study, uncontacted families for whom definite information was lacking as to a change or residence were returned to the pool.

4. Interviews Completed. Of the 771 interviews attempted, 573 (74%) were contacted and completed.

5. Statement of Intent to Participate. Those parents meeting our criteria, including that of English being the dominant language spoken in the home, were asked to sign a statement of intended participation indicating they could and would participate.

6. Orientation and Scheduling. From two to six weeks following the interview, the mother and child were invited to the Harlem Research Center of the City University of New York where the testing was to occur. At the Center the mothers, in groups of two to five, were further oriented to the study, while the children began the adaptation process preceeding assessment.

Table 1 shows attrition rate by stage of selection and by social class.

The data of the selection process revealed that among those identified from birth records as to social class, the lower class parent was slightly less easily located and interviewed than her middle class peer. She was slightly more likely to sign the statement of intent if interviewed and slightly less likely to actually participate having signed that statement. If contacted by interview, she was slightly more likely to participate in the program (44% to 40%).

We concluded from the analyses that no bias in the sample existed with respect to middle and lower class at the different stages of the selection process, and that the subjects used in the intervention research for which they were destined were appropriate for the questions asked by the design.

The steps involved here and the analysis to determine whether bias existed in the selection of our Lower Class and Middle Class groups required a significant investment in time and energy. Without such steps or similar ones, however, one is constantly exposed to the criticism that differences between groups may have been a function of sample selection rather than the treatment concerned.

III. RESEARCH DESIGN

With few exceptions intervention studies are designed with a treatment group and a single control group, the latter being seen for assessment only. It is as if no one had ever bothered to create or discuss what is known as the Hawthorn

Table 1. Attrition from subject pool by stage of selection and initial social class.

Stage of Selection	Social Class						Total	
	Middle Class		Lower Class		Unknown			
	N	Percent	N	Percent	N	Percent	N	Percent
a. Birth records copied	333	23	544	37	584	40	1461	100
b. Address confirmed by Post Office	239	72	353	65	435	74	1027	70 of a
c. Interviews attempted	192		296		283		771	
d. Interviews achieved	152	79	221	75	200	71	573	74 of c
e. Signed statement of intended participation	115	76	178	81	162	81	455	79 of d
f. Attended orientation (scheduled child)	91	79	123	69	109	67	323	71 of e
g. Attrition from d to f	61	40	98	44	91	46	250	44 of d

effect. For those who have forgotten their industrial psychology, the Hawthorn effect states simply that a subject in a study performs better just because he is a subject in a study and knows it. When the Hawthorn effect is in force, a treatment group may show better performance for reasons apart from what the investigator thinks he is doing. Let me illustrate: A madam hypothesizes that were she to paint the walls of her girls' bedrooms red, she would increase the number of customers serviced every night. She paints the walls red, lets the girls know about her experiment as well as the fact that they are part of it, and production soars. She concludes that red walls have in some way affected the autonomic nervous system of the customers, which in turn has led to higher production, when in fact the girls are hustling because of their newly perceived importance of being part of the madam's experiment. Even if the madam had had some knowledge or research design, and had instigated a single control whose rooms were not painted or changed at all, and whose girls were not informed that an experiment was taking place, she may have come to the same conclusion, since no rise in production presumably would have occurred in her control group. Only with a second control group, one in which the madam did not paint the walls but told the girls that they were in an experiment and being observed, could she have had the comparison groups necessary to conclude that indeed it was the walls that had turned the trick.

It is the second control group which is all too frequently omitted in intervention studies. It may be no more than a modified Hawthorn effect when we show improved performance in preschoolers over the non-participating controls, regardless of the treatment used.

It has been our experience that the second control group is not only good research design, but provides certain information related to the question asked by the study which is otherwise impossible to obtain. When we designed the Harlem study, we included a participating and a non-participating control group to control for the Hawthorn effect. Our three groups were as follows: First, there was our experimental training group, which used a curriculum laboriously contrived and absorbing hundreds of hours of effort. The curriculum developed, in sequence, increasingly complex concepts beginning with simple concepts such as on top of, open and close, etc., increasing with complexity to such things as one, many, some, different, and so forth. Special materials were chosen to illustrate the concepts which were used. The sessions lasted 45 minutes twice a week for eight months. Our <u>participating control group</u> was selected randomly from our subject pool, and the subjects were treated the same as our experimental training group except that they were not taught concepts in

the curriculum. Nor were the concept words used at any time in the situation. The instructors were informed that they were not to initiate conversation with children in the participating control group.

Our non-participating control group were children from the same subject pool who would be assessed annually but had no contact with the laboratory during the intervening periods. The third group is, of course, that which most intervention studies include. You will note that it is the participating control which is different from most designs.

While the participating control group was originally included in order to control for the Hawthorn effect, it soon became apparent that the children in the participating control group were progressing very well indeed. Remember, they had the same materials, the same instructors, the same conditions as the experimental training group, but they were not exposed to this ingenious and highly developed curriculum which my staff had put together. After eight months of exposure to our program, all children were given a battery of 17 measures which assessed their performance on several domains of behavior. The average testing time was six hours, spread over six visits to the center. We were delighted and not surprised to find that our experimental training children outperformed our non-participating controls on 15 of the 16 measures administered immediately after training. (I might add, parenthetically, that a year later with no subsequent intervention the experimental training group was still superior on 10 of the 15 measures administered.) Thus, we have shown what most intervention studies show: Our treated group had outperformed our non-participating control group. However, it was also true that the participating control group outperformed the non-participating control on 14 of the 16 measures administered immediately after participation in the program. Now the conclusions we may reach are very different from what we would conclude had only the treatment group and a single control group existed. Compare our curriculum and training procedures to the madam's red wall: Only because of the presence of the participating control group can we conclude somewhat unhappily that it is not the curriculum which brought about the change since the non-participating controls did practically as well. We now hypothesize that the effects of our program are more related to the one-to-one relationship and the affective bond developed between child and instructor during the regular and uninterrupted training sessions, than to our curriculum. It suggests that our curriculum stressing basic concepts may be no better than anyone elses where a similar investment of time and thought

was made, that is, that one whose rationale was manifested by increasing vocabulary size and the ability to label objects could provide similar results if the training conditions we used were included. Our present emphasis on the conditions of learning at age two and the development of the affective relationship as compared to a curriculum stressing concept learning is the direct result of having included the participating control group as a control for the Hawthorn effect.

IV. STAFF TRAINING AND EVALUATION

A variety of things can occur to increase the logistical complexity of an intervention study. As we said before, such occurrences de-emphasize the research aspect of the study, and add to the possibility that the resulting data may become useless. This is especially true in dealing with the personnel who work directly with the children. Staff training and evaluation, if not properly controlled, can ruin research:

1. The Continuous Need for Training. In any study where a large number of instructors are involved, a turnover will occur. Few of us can devise sufficiently intellectually challenging teaching situations for the two and three year old so that it also holds the interest of intelligent instructors over an extended period of time. While this depends on the nature of the curriculum or program concerned, it is frequently the case that instructors turn over simply because they are bored with doing the same kinds of things with children every day. Consequently, we are continually in the training business, that is, training of instructors.

2. The Continuous Need for Checking. Associated with the continuous need for training is of course the continuous need for checking, for observing instructors. No matter how well trained the average instructor is, he will tend to become careless unless he is continually observed and unless the goals of the study are continually put before him. We have used two ways particularly to know what our instructors are doing and to keep them alert. The first of these is by doing actual studies as subsets of the larger study, that is, studies of instructor behavior. The second is by comparing kinds of data which are already collected to determine the relative performance of different instructors. An illustration of the smaller studies done as subsets of the larger study is where we check the amount of verbal behavior initiated by the instructor. You will recall that instructors working with children in the participating control group were asked not to initiate conversation with the child, and only respond to the child if the child initiated the conversation. In order to determine whether in fact the instructors did this, we did a series of studies, checking the content and frequency of verbal

interaction between instructor and child. In our case, our instructors were following their mandate and we showed conclusively that the participating control group did not have anywhere near the amount of verbal interaction with instructors as did our experimental training group. The comparison of existing data is also a good way of checking instructors. If progress reports are kept up on the children and each instructor sees several children or several classes, quantitative judgment about a given child are possible provided that an instructor is dealing with enough children. The range, average, and variability of performance of the children working with her can be compared with her peers. Precise analysis can be performed to determine whether or not any given instructor is getting performance outside of what would normally be expected by chance.

3. Assessment and Instruction. One of the most frequent errors performed by intervention studies is that the instructorial staff also operates as the assessment staff. Anyone who has spent much time around preschool children realizes that both children and instructors are easily seduced and have many love affairs. It is not therefore surprising that we would recommend strongly that the assessment procedures which are used to determine whether or not a treatment has been effective be administered by test personnel who are entirely separate from the instructorial personnel. The further these two groups can be kept apart the better. And it is relevant for the assessment group as well as for the instructorial group that they know as little about the design of the study as possible. That is to say, either instructors or assessors who are working in a study which is co-varying social class and treatment may bias their testing or their instruction if they know they are dealing with a lower class or middle class child.

V. ASSESSMENT

Presumably, preschool intervention programs aimed at the two and three year old will increase in number and importance as evidence for the positive effects of those programs increases. From this we may infer that there will be a concomitant increase in the need for measures by which we may evaluate programs, and which will aid us in the diagnosis and educational planning for individual children as well. To a significant degree the nation will get a return on its investment as a function of how well those measures service the needs for which they are developed.

Measurement of the child's performance before and after a treatment of any kind is perhaps the most important phase of any intervention program. No amount of evangelical zeal

can replace hard data showing that performance changed on specific test scores. Measurement is not only important to identify change but to specify what dimensions of behavior changed as a result of the treatment and what dimensions did not. The feedback it gives one with respect to the effects of the investigator's dream can be extremely fruitful if he is willing to accept the results--if he is willing to see it like it is. I would like to speak to three aspects of measurement which are particularly relevant to intervention studies: the comprehensiveness of the measures used, the period of adaptation a child requires before he is tested, and experimenter bias with young children.

1. The Comprehensiveness of a Test Battery. Since most treatments (or curriculum) in intervention studies usually deal with diverse dimensions of behavior, we may assume that the dimensions of change in the child are not all known. Consequently, it behooves us when possible to measure the child in as many domains of behavior as we can. There is a tendency for intervention studies to only use standardized measures simply because they exist. These tests are valuable, particularly as anchor items. However, the use of two or three measures does not provide an assessment of enough of the child's behaviors to get a systematic picture of the treatment effects. The measures which we used are:

A. A Concept Familiarity Index, which measures the child's knowledge about simple relationships between himself and his environment. We began with simple concepts such as open, close, on top of, rough, smooth and increased the difficulty of the items to include same, different, one, many, and so forth.

B. The Stanford-Binet IQ.

C. The Peabody Picture Vocabulary Test.

D. A measure of Persistence at a Boring Task, which we have designed to try and find out if we effect the child's ability to work with a strange adult.

E. The Embedded Figures measure.

F. A Sequence Test, in which we measure the child's ability to follow instructions.

G. Simple Perceptual Discrimination.

H. Motor Performance.

I. Labeling.

J. Location Discrimination.

K. Positional Discrimination.

L. Simple Form Discrimination.

M. Varied Form Discrimination.

N. Color Discrimination.

O. A measure of knowledge about Body Parts.

P. A measure of knowledge about Body Positions.

Q. A measure of Delayed Reaction.

R. Varied Form Puzzles.

S. A Grouping Task.

T. A Sorting Task.

More diverse batteries for assessment will provide us with greater information about the generalization of a given treatment. That battery will also help us to identify behaviors not affected by the treatment. Finally, a more comprehensive battery has another great advantage. Where systematic bias in testing exists, it will frequently be the case that if effects the results of test A more than test B. Let me illustrate: The writer and one of his students has found at age three that the IQ's of 120 Negro males correlated -.51 with a measure of degree of frustration upon separation from the mother. The higher the frustration, the lower the score, even though the measure was not administered until after the subjects had met a criterion of readiness for testing. Other measures from our battery such as the Peabody Picture Vocabulary Test correlated -.21 with the frustration measure, and our Concept Familiarity Index correlated -.29. Thus, we have information which suggests that at age three some performances are more effected than others by the frustration of the child in the testing situation. We also found that Binet scores correlate -.54 with this frustration measure at age three suggesting that the Binet is one measure most effected by the child's level of adjustment. Presumably, we would need a longer period of adaptation to the testing environment before the Binet can be administered than for other measures.

2. Adaptation to the Testing Situation. A significant proportion of two and three year old children will not respond to the examiner's request in the test situation until

a period of adaptation to the testing environment has occurred. The individual differences among children during this adaptation period are great. Standard operating procedures must be developed for any study with criteria to be met before testing begins. Only then are we reasonably confident that the child is performing up to his capacity. Our criteria for testing are: No child is ready for testing if the mother is in the testing room, or if he manifests anxiety about her absence when she is not present. No matter how many adaptation sessions are required before testing begins, the mother must be out of the testing room and the door must be closed. The examiner must judge the child to be sufficiently adapted to the situation before testing may begin, and a senior examiner who has observed the child and the examiner must confirm that judgment. Children vary in their rate of adaptation using these criteria. Some are ready for testing in the first session in which they come to the Center; others take seven or eight visits. With this criterion, there is a low negative correlation between the number of visits it takes to adapt and test scores.

As we accumulate norms about these measures and the extent to which they are influenced by variables associated with experimenter bias, we are more and more satisfied that in fact we did measure a broad range of the young child's true behavior reliably. We believe such an array to be a distinct advantage over the use of two or three standardized measures on which are based the effects of a given treatment.

3. Experimental Effects. Taken together, the many general and specific means by which an experimenter may influence his subjects' behavior are complex and often difficult to identify. Over the past four decades, however, an increasing amount of information on experimenter effects in behavioral research has become available.

Rosenthal (1966) summarizes the evidence for experimenter effects and discusses in detail the critical attributes of experimenters which may determine a subject's responses: nonspecific experimenter variables such as age, sex, race, religion, relative social status; the investigator's personality characteristics (e.g., his customary levels of anxiety or hostility, his warmth and need for approval) and intelligence; and the channels of communication employed (e.g., visual, auditory, motor) have all been found to affect the behavior of animal and human subjects. These experimenter characteristics are complemented by situational factors: the experimenter's experience in his role, i.e., whether he has had prior contact with his subject; the subject's behavior; the influence of the principal investigator's set, attitude,

or hypotheses; and the specific environment in which the research occurs.

All of these experimenter variables have not been examined equally often, and some effects have been fortuitously revealed. Nevertheless, research findings suggest that, with respect to the process by which the effects are induced, the experimenter's demand characteristics (Orne, 1962) and his subtle communication of what kind of performance he expects from the subject, regardless of the stated instructions, combine to affect the results. Experimenter effects may also be passive or active but, again, it is difficult in practice to distinguish with certainty the different influences of an experimenter's degree of hostility, his visual and motor behavior, his age and/or sex in the experimental situation. In spite of these obstacles, attempts to specify the amount of subject error variance derived from experimenter effects have met with some success in measurement, though, on the whole, little or nothing has been done with measures associated with two and three year old children.

The effects of experimenter bias may be more or less severe with preschool children than has been found for measurement generally, but whatever the case may be, it is clear that it is probably a powerful variable associated with testing the very young. To use measures which tap only a limited amount of the child's intellective repertoire, or to use measures where every conceivable attempt to avoid experimenter bias has not been made is to penalize children generally and to avoid some of the more crucial questions confronting us about the effects of early childhood education.

RESULTS AND DISCUSSION

This has been something of a whirlwind tour through the problems associated with doing intervention research. It may be that I have given the impression that I feel that all of those problems have been solved at the Harlem Research Center, and some of them have. However, to close on a note of humility, let me tell you of something that occurred in my lab two weeks ago, something so terrible to discover that the only thing I can imagine to have been worse is if we had not discovered it at all.

Presently, we are comparing the performance of two and three year old children of Negro, Puerto Rican, and white ethnic backgrounds, using the controls and measures that I have described. Needless to say, with so sensitive a subject, we have been even more stringent about our caveats

than I have heretofore suggested. We have been testing for four months, with Puerto Rican and white children matched for our already existing Negro sample by education of parents and occupation, matched child for child that is. The results on the first 20 Puerto Ricans showed that they were doing very well indeed with the measures we had translated into Spanish. They were equal to or better than the whites on the Stanford Binet, for example.

But two weeks ago, over a cup of coffee, one of our Puerto Rican assessors said to the senior instructor at the laboratory: "We still don't understand why Dr. Palmer assumes that Puerto Rican children are more stupid than others." The senior instructor almost fell out of her chair: "Whatever gave you that idea?" The story unfolded as follows: When the Puerto Rican assessors had just completed their training on the measures we use, a young man from the computer center at the university took some data to the lab and was talking with the pretty Puerto Rican girls. "What does Epsilon mean?" he was asked; "Stupid or slow," he responded.

It seems that we have the practice of designating subject samples from study to study by Greek letters: Alpha, Beta, Gamma samples are part of our intervention study; Delta and Epsilon samples are part of our ethnic comparison study. The Puerto Rican sample happened to come up with Epsilon, but it could have been number 5 or capital letter E or Green, depending on our choice of symbols.

The young man in the computer center was a creative writing undergraduate deeply immersed in Brave New World in November when the assessment started. Brave New World, as you know, has an Epsilon group who were the workers, more stupid than others. Entirely unrelated to the study, he has responded with his most recent association with the word epsilon, and had imparted that knowledge to the new Puerto Rican assessors when they asked, entirely uninformed about the study as he was.

The result was that we do not know whether the incident brought about an experimenter bias which moved the Puerto Rican scores up, and will not know until we have trained new assessors, selected new subjects, assessed them and compared the results to the data on hand. A conservative estimate of cost is four months' work and twelve thousand dollars. How might one control for that one?

THEORY AND POLICY OF

PRESCHOOL INTERVENTION PROGRAMS

Francis H. Palmer

With the recent change in administration, all of us here are curious about what the new administration's policy is going to be towards social problems, in general, and towards preschool education, in particular. What, if any, changes are going to be made and what, if any, new programs will be initiated. Most of us would agree that we know enough about children to be able to improve their intellective, social, and emotional growth given the opportunity. We might differ on the details of how to do it, but we could do it. The administration must realize that to be effective on a wide and meaningful scale, the government is going to have to spend large amounts of money. Small research ideas or slight tinkering in the machinery isn't going to make much difference. We need billion dollar ideas that will work. Our strategy to our Congressmen should be that it is not a question of whether or not we can do something for children; it's a question of how much does this society want to do for children.

In mapping out a billion dollar preschool program, the role of the ghetto mother in addition to that of the child should be considered. Two myths often surround the ghetto mother: One persists that she is poorly motivated with respect to her desire to improve her two to four year old child's development; and second, she's not well educated enough to handle her preschool child.

Data from the Harlem Research Center has shed light on these myths, showing in fact that they may not be true. We found no difference in the number of mothers of lower socio-economic background who enrolled in the project as compared with mothers of middle socio-economic background. Further, once enrolled, the lower class mother saw to it that her child came to the Center as often as the middle class mother did. After 7,000 visits by our two year olds, there were no differences in the attendance of lower and middle class children over a period of eight months. Thus, in volunteering for the program and seeing that the child continued to participate, the lower class mother appears as

motivated as the middle class mother. Someone challenged me on this use of the word motivation in class the other day. Call it what you may, but attendance does indicate that the mothers were interested in their child's development. Other studies, however, have shown that by the time a child gets into grade school, ghetto mothers become less involved in their child's development. A student of mine has proposed doing a study with those mothers in our sample who have older children in school for the purpose of studying how involved these same mothers are with a child of school age. What we predict is that the same mothers will show a difference by social class in motivation with regard to their school age children where they did not differ in their motivation towards two and three year olds. If we are correct, this discrepancy may be due to the fact that the mother of the two to four year old feels she still has control over her child's development. Whether that prediction is correct or not, we still know that the ghetto mother of the two to four year old is, in fact, motivated as much as her middle class peer.

The second myth is that the ghetto mother of a two to four year old, usually poorly educated herself, is incapable of providing the increasingly complex stimulation required for development of her child and consequently his intellective development suffers. If this were true, one would expect that children from middle socio-economic families would perform better on tests of intellective ability than children from lower socio-economic families. However, no differences were found, in general, on our battery of sixteen measures between lower and middle class Negro boys from age two years to three years and eight months. This finding runs contrary to the generally held belief that differences by socio-economic status do exist with children under four. However, most of the data showing differences between children from middle and lower class backgrounds has been done with kindergarten or school age children, and these differences were assumed to be found with younger children. It may be that our careful selection of subjects or pretest adaptation sessions, as described in the adjoining paper, give a truer picture of the lack of differences between young children of differing socio-economic backgrounds. Our data suggests that the educational level of the ghetto mother may, in fact, be adequate for providing appropriate stimulation during our first four years.

Taking into account our findings, the assumption can be made that the ghetto mother is (a) motivated to do something about her two to four year old, and (b) that she is sufficiently educated to take care of her child at least until the age where no differences by socio-economic status in intellective performance are shown. I, therefore, hypothesize that the

mother of the two to four year old child is a powerful and unexploited tool for social change. In recommending that the administration institute on a large scale programs for two to four year olds which have been shown to work, I further urge that attention be given to the ghetto mother of these children. Take her off welfare, pay her for a job she can do well, namely, teach young children, and give her a sense of pride and dignity. For example, Dunham is working with lower class mothers and teaching them to use our curriculum we have developed with their own children. They could also be used to teach other children as well. I am further suggesting that the specifics of a program in a given geographical area could differ because each investigator knows his area's problems best. However, the children should be assessed every year on a group of comparable measures so that comparisons across geographical areas could be made.

In summary, we can help the intellectively, socially, and emotionally deprived child. His mother is motivated toward and capable of participating at least to age four. The limitations on our accomplishing those goals are more a function of how much the American people will spend than our ability, the mother's, and the child's.

PHASES IN GROUP PROBLEM-SOLVING: A SLIGHT REVISION

Fred L. Strodtbeck and Ellen Kolegar

INTRODUCTION

My understanding of the way my remarks fit in this program is as follows: If there are to be national programs, there will be responsibilities to appraise the effects of these programs on family functioning. Given, say, recorded samples, or other knowledge of the interaction in a family, there is a question of the changes in this interaction that might be brought about by the new program on such matters as community involvement, or special training in achievement attitudes.

Some of you are familiar with a contribution in the volume, Talent and Society, edited by McClelland.[1] In this I discussed the effects of the allocation of power in Jewish and Italian families on conceptions of destiny. I spoke of trends which tend to be recreated, generation after generation, in these ethnic groups. I've more recently been concerned about the kinds of familial experience which causes a person to have an effective self-interest in a power situation. Some of you are familiar with the paper, "The Hidden Curriculum in the Middle-Class Home."[2] More recently, I've been working on the effects of various allocations of power and support in the family for sexual identity.[3]

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[1]Strodtbeck, Fred L., Baldwin, Alfred L., Bronfrenbrenner, Urie, and McClelland, David C., Talent and Society (New York: Van Nostrand and Co., 1958).

[2]Strodtbeck, Fred L., "The Hidden Curriculum in the Middle-Class Home," in Urban Education and Cultural Deprivation, edited by C. H. Hunnicutt (Syracuse: Syracuse University Press, 1964), 15-31.

[3]Strodtbeck, Fred L. and Creelan, Paul G., "The Interaction Linkage Between Family Size, Intelligence, and Sex-Role Identity," Journal of Marriage and the Family, May, 1968, 301-307.

Today, I'm going to address myself to what is perhaps a still more fundamental linkage in this relationship, namely, the theoretical perspective through which we feel that interaction is translated into implications for personality development, self-concepts, and related constructs of the type we draw upon to encode our guesses about the long-term implications of prior experience.

In the present paper, husband-wife interaction over revealed differences are presented as particularly clear instances of the type of "full-fledged" group problem required by the Bales-Strodtbeck phase hypothesis. Two independent sets of such materials are used. These confirm the essential validity of the original formulation. However, through the clearer profiles of the relative category usage which they provide, a heretofore unanticipated eruption of negative reactions in the second sixth of the problem-solving cycle is demonstrated. The hypothesis as to the successive emphases was stated in the original paper in five rank order statements. It is suggested that this be replaced with empirical norms for sixths of a meeting cycle for each of the 12 interaction process categories. This change increases both the precision of the hypothesis and the ease with which it can be translated into movement in the three dimensions: power, liking, and achievement.

In the earlier literature purporting to describe an order which would make group process most effective, there was an uneasy recognition that, for different types of problems, different orders might be required; see Dewey (1910), Elliot (1928), and Lasker (1949). In 1951 Bales and Strodtbeck described a tendency for groups to move through various emphases in terms of the relative use of interaction process categories. Thus, there was a joining of the idea of phases with an empirical method which would give them operational substance. The important step in moving toward greater precision in specification was the suggestion that the phase hypothesis applied only to full-fledged problems. What was meant by full-fledged was specified in terms of concepts which, in turn, were part of the definition of the categories. In essence, the concept requires that the functional problems of orientation, evaluation, and control be unsolved at the beginning of the observation and solved, in some degree, during the period of observation.

The functional problem of orientation relates to the degree to which members of the group are ignorant and uncertain about the relevant facts and to the distribution of facts relevant to a decision. The problem of orientation will be greatest when uncertainty is high and the relevant information widely distributed.

The functional problem of evaluation arises when the discussion involves different values and criteria by which the facts of the situation and the proposed course of action can be judged, and when members possess different values and interests which must be reconciled before reaching a conclusion. The problem of evaluation will be greatest when the problem itself is many faceted and the interests or perceived utilities of members are distributed so that they cannot be simultaneously maximized in a given solution.

The functional problem of control relates to the pressure for group decision and the expectation of further joint action. The problem of control will be greatest when the pressure for a decision is great and when the members have some uncertainty about their respective role prerogatives. To the degree that the decision in question is seen as having implications for subsequent role performance and for the state of integration which will characterize the group as it moves to consider additional problems, the problem of control will be accentuated.

In the original article, the authors suggested that the hypothesis should work for persons with normal intelligence who are not suffering from serious personality disturbances. They excluded groups which had some formal procedure, or members with some personal strategy, which if acted upon overtly, would circumvent the phase pattern.

I. RECONSIDERATION OF PHASES

The authors had anticipated that, by providing the Bales interaction process categories and the statement of the phases associated with full-fledged problem solution, others might be motivated to work out characterizations of phases for other conditions such as therapy o- sociability interaction. New formulations, however, were not proposed. The paper, unfortunately, was simply reprinted (Cartwright and Zander, 1953; Harris and Schwahn, 1961; Lazarsfeld and Rosenberg, 1955; Swanson, Newcomb, and Hartley, 1952). The hypothesis came to be regarded as a simple, sovereign formulation.

The objective of the present paper is to reopen the phase question in the limited perspective of a detailed analysis of a set of two-person data which are believed to unequivocally meet the condition of the original hypothesis. In approaching the hypothesis in this way, it will be possible to clarify three minor shortcomings in the original paper: (a) The empirical norms presented include both the groups which did and those which did not fulfill the requirements of the full-fledged problem;[1] (b) Data for the discussions were divided into thirds, instead of finer divisions;[2] and

(c) Some interaction process categories were grouped together which were anticipated to have slightly different phase peaks and which would, therefore, need to be separated if finer time divisions were used. In this paper, independent sets of data provide a further empirical check on the correctness of the hypothesis, and response to the above shortcomings makes possible the attainment of greater precision in its statement.

II. SOCIABLE INTERACTION

Recent work by Riesman, Potter, and Watson (1958, 1960a, 1960b, 1962) on sociable interaction has made clearer why limiting the original formulation to the full-fledged problem situation was necessary. They stress the paramount importance in sociability interaction of self-definition. They describe two types of participation: presenting, when a participant establishes his unique identity by performing against the facade presented by others; and sharing, instances in which the self-systems of participants fuse vis-a-vis others whose activities are under review. In the sociable interaction they describe, the shift from presenting to sharing proceeds by episodes which involve shifting alignments and locations of actors. In lieu of movement toward a goal, there is a movement away from the overt topic (called a resource) through elaborations which enable members to present themselves and thereby achieve covert objectives relating to self-definition. In sociable interaction, a given topic is little more than a point of departure and, although it may be returned to repetitively, it is not resolved. In the full-fledged problem, one problem is recognized and resolved in the topical cycle.

III. REPLICATIONS OF PHASE INVESTIGATION

Landsberger (1955), in his study of interaction process during the mediation of labor-management disputes, demonstrated not only that the phase hypothesis was confirmed, but that in the mediations in which it was most clearly present, the parties most completely resolved their differences. Olmsted (1954), working with groups who had either been instructed to pay particular attention to getting the task done or to be sensitive in interpersonal relations, reports that the phase hypothesis was confirmed in both cases. The tasks used in these investigations appear to have fulfilled the full-fledged problems requirements.

In therapy groups, which would not be assumed to work at full-fledged problems, as defined above, the phase effect has not emerged consistently. Negative findings are reported

by Talland (1955), Lennard and Bernstein (1960, Ch. 3), and Smith, Bassin, and Froehlich (1962). Psathas (1960), in later criticism of Talland, shows that Talland had a concentration of acts in Category 6, Gives Orientation, which would cause one to believe that interaction process had not been scored in the conventional way. Psathas demonstrates that, when the therapist's interaction is included in the analysis, the phase-sequence is found. He uses a sample of early, middle, and late sessions in each of two psychotherapy groups which met over a period of a year.

Lennard and Bernstein cannot be considered to be a clear disconfirmation because they did not use interaction process categories. A rough approximation of distinctions in interaction process categories does, however, exist in the categories they used and, when evidence for the phase hypothesis is sought, no confirmation was indicated. Smith, Bassin, and Freohlich appear to use the interaction process categories in the standard manner. Their summary profiles were not markedly different from those observed in other groups save for an atypically high concentration in Category 6, Gives Orientation. They conclude that the interaction within the probation groups were probably more like a therapy (Talland, not Psathas) than a problem solving group and, for this reason, the phase hypothesis was not supported. Thus, there is no clear indication that group therapy interaction does, or should, follow the phase hypothesis.

As we have indicated earlier, the summary table of the original article included truncated, as well as the full-fledged, group interactions. Since the hypothesis worked for us despite the heterogeneity (as it does to some degree for Psathas in therapy groups), others were encouraged to look for it under almost any condition. By returning to a set of data which is more consistent with the required conditions, the dialogue between investigators can be taken up with prospects of a clearer understanding of both limitations and strengths of the phase formulation.

IV. PROCEDURE

To obtain social units with long-term expectations of working together, ten families were selected by lot from the post office lists in each of two villages in New Mexico (see Strodtbeck, 1951). Each couple was asked, in the presence of the experimenter, to pick three families with whom they were well acquainted. It was suggested that these reference families should be of similar socio-economic status, not be more closely related to the husband's family than to the wife's, and should--as the couple who were used--all have children. The husband and wife were then separated and

requested to report privately which of the three reference families most satisfactorily fulfilled a series of 26 conditions such as: Which family has the happiest children? Which family is most religious? Which family is most ambitious?

These were questions which were believed to touch upon the normative consensus which would guide the husband's and wife's expectations concerning their own family functioning. After both husband and wife had individually marked their choices, they were requested to talk over how they came to differ and, if possible, reconcile their differences in order to indicate a final "best" choice from the standpoint of their family. In one of the communities, the recording of differences took place in the school house (where electric power was available) on a Saturday afternoon. In the village, six of the families carried out the discussions in their own living rooms--a recording system powered by a Jeep was used. In four cases the location of trees and irrigation ditches was such that a Jeep could not be brought alongside the house and, in these cases, the couples carried out the reconciliations in their own cars. In all cases the reconciliations were recorded under conditions insuring privacy.

The discussions were transcribed and the written transcripts were scored in interaction process categories by B. F. Bales. The scoring was done entirely from the written protocols; no use was made of the recordings. The number of acts in each discussion was obtained and each act was identified on a separate IBM card in terms of its percentile position in the total interaction. The number of discussions per family ranged from 3 to 13. A total of 5,205 acts were produced by the 20 families; the five discussions in which there were fewer than 15 acts are not included.

V. FINDINGS

In the original hypothesis the essential idea is that the need for orientation precedes the need for evaluation and both of these precede the problem of control. Since there are ordinarily no inherent grounds on which to divide interaction scores into phases, the criterion of thirds was used in the original study and sixths will be used there. Table 1 shows that 6, Gives Orientation, and 7, Asks for Orientation, when taken together, clearly confirm, in terms of relative rank by thirds, the expectations of the first paper. Save for the one inversion between the first and second sixth for Category 7, the shift to sixths is consistent also, but this slight exception in no way reduces the strong confirmation of the original prediction.

TABLE 1

FULL-FLEDGED HUSBAND-WIFE INTERACTIONS[a]
COMPARED WITH PHASE HYPOTHESIS PREDICTION

Bales IPA Category	Prediction by Thirds	Phase Values (in sixths) 1st	2nd	3rd	4th	5th	6th	Total Acts
6 Gives Orientation	Hi-Med-Lo	27	18	16	14	14	11	1393
7 Asks for Orientation	"	20	23	17	15	14	11	516
5 Gives Opinion	Lo-Hi-Med	12	16	19	20	19	14	1336
8 Asks for Opinion	"	27	14	15	22	13	9	211
4 Gives Suggestion	Lo-Med-Hi	9	9	7	10	18	47	141
9 Asks for Suggestion	"	5	10	20	20	30	15	20
1 Shows Solidarity	Lo-Med-Hi	11	4	9	5	22	49	97
2 Shows Tension Release	"	11	16	14	13	17	29	296
3 Agrees	"	9	11	16	18	21	25	756
10 Disagrees	Lo-Med-Hi	15	24	17	20	14	10	322
11 Shows Tension	"	11	17	13	15	22	22	46
12 Shows Antagonism	"	4	18	21	14	10	33	71

[a]Based upon 149 revealed difference interactions by 20 families.

For 5, Gives Opinion, and 8, Asks for Opinion, the rationale predicts a peaking in the middle periods and the present data again are in conformity with the predictions for both categories save that Asks for Opinion, when viewed alone, also has its absolute peak during the first sixth. This was not previously anticipated. It causes the Low, High, Medium predicted to become Medium, High, Low by a small margin.

For acts of control, 4, Gives Suggestion, and 9, Asks for Suggestion, the frequencies are low--only 3% of the total acts. For the present data, as for the original data, even though the sum is low, the trend for the categories combined, as indicated in Table 1, is clearly in accordance with the prediction whether viewed by thirds or by sixths.

It was predicted that 1, Shows Solidarity, 2, Shows Tension Release, and 3, Agrees, will also start low in the first third and rise to their peak in the third period. It has been anticipated that laughter and joking at the end of the discussion releases in diffuse ways the peak of tension which occurred at the time when the control efforts were most strongly mobilized. Thus, negative reactions (10, Disagrees, 11, Shows Tension, and 12, Shows Antagonism) were predicted

to reach their peak just before the control efforts which are attended by the positive reactions which end a given cycle. One might have made essentially this same prediction by assuming that a group gets "warmed up" and produces more affective behavior after the passage of time.

It may be seen in Table 1 that positive reactions (1, Shows Solidarity, 2, Shows Tension Release, and 3, Agrees) do fulfill the phase prediction. In the absence of truncated problems, the gradients are more sharply differentiated between the three phases than in the early data. When the detailed breakdown in Table 1 is examined, we see that laughter is clearly accentuated in the last sixth and there is a previously unrecognized heightening of acts of solidarity in the first sixth. These departures are minor, and 3, Agrees, shows the predicted increase in particularly clear form.

For negative acts, 10, Disagrees, 11, Shows Tension, and 12, Shows Antagonism, the prediction is again correct in composite form, but when viewed by sixths, the adjacent values do not continuously increase as we had originally expected.

VI. A COLLECTIVE AWARENESS PHENOMENON

The flurry of negative reactions in the second sixth of the discussion may possibly constitute an analogue of what has been found in individual problem solving--namely, a preconscious functioning. Sidney J. Blatt (1959) has demonstrated on the PSI apparatus that there is a significant increase in cardiac rate at points when the subject has available sufficient information for problem solution and again when he shifts from one-to-one relatives to more complex synthesis questions. Since he cannot get verbal reports from his subjects which show their conscious awareness of these stages, he interprets his data as evidence for an existence of intuitive, affect-laden, thinking--a preconscious functioning in efficient problem solving.

In the present instance we have the feeling that the disagreements which are made at the end of the orientation period are in part a search for clarity about the situation. It is as if the participants perform more spontaneously before they know the exact alignment. It is not entirely a matter of knowing the alignment--because that is stated by the experimenter. It is more a matter of knowing the degree to which the respective others hold to their answers. As soon as the alignment is, in this sense, known, the task becomes as much one of how to lose gracefully as it is how to win. The preconsciousness in question relates to the

psychological reactions which must be mediated into logical actions to complete the interaction. Even though the display of choice and commitment may have arisen in an atmosphere which overtly suggests that the question is still "open," the strong, briefly unsuppressed, negative responses indicate that some participants recognize that "openness" is no longer present.

VII. THE MEASUREMENT OF PHASE CONFORMITY

In the original paper, a cumulative transposition measure (see page 491) was described to represent the conformity between a new set of data and the hypothesis. The significance levels given were computed by an approach which required an independence between sub-hypotheses which probably was not met. This difficulty still persists--an eruption of frequencies in any one category exercises a constraint in all other categories. We do not know how to take this constraint into account theoretically but, by correlating the values in Table 1, we can provide two empirical observations:

First, the average correlation in the total matrix is zero, see Table 2. This means that the perfect negative correlation which would be observed if X and T-X were correlated in a two element system has been attenuated. The observation of a zero average correlation, when intercorrelations between this twelve element system are involved, indicates that dependence is not serious in this instance.

Second, by arranging the rows and columns of the matrix in a cannonical form which maximizes the closeness of the positive correlations to the main diagonal, we can demonstrate that the original grouping of categories into subsets for interpretation was not wise in at least three instances:

(a) Category 5 with 8, Gives Opinion and Asks for Opinion ($r = -.20$);

(b) Categories 10, 11, and 12, Disagrees and Shows Tension ($r = -.40$), Disagrees and Shows Antagonism ($r = -.28$), Shows Tension and Shows Antagonism ($r = .49$);

(c) Category 4 with 9, Gives and Asks for Suggestion ($r = .08$)

TABLE 2

THE INTERCORRELATION OF IPA

CATEGORIES (TABLE 1) IN CANNONICAL FORM

Bales IPA Category	Bales IPA Category										
	4	2	3	11	12	9	5	10	6	7	8
1 Shows Solidarity	98	92	77	70	64	14	-35	-85	-50	-76	-61
4 Gives Suggestion		96	78	73	72	08	-33	-73	-56	-73	-62
2 Shows Tension Release			77	78	84	12	-23	-58	-66	-64	-79
3 Agrees				79	63	66	31	-62	-88	-48	-68
11 Shows Tension					49	54	14	-40	-75	-60	-81
12 Shows Antagonism						06	00	-28	-72	-48	-78
9 Asks for Suggestion							81	-19	-68	-59	-43
5 Gives Opinion								34	-57	-23	-20
10 Disagrees									21	76	26
6 Gives Orientation										72	14
7 Asks for Orientation											43
8 Asks for Opinion											

While there appears to be no practical objection to the other pairings (i.e., 6 with 7 and the set of 1, 2, and 3), it is our present feeling that correspondence to the phase hypothesis should be approached without pooling categories. We believe that new data should be arranged in the form of Table 1, then correlated, row by row, with the values in Table 1. To combine the row correlations, a weighting by values which are proportional to the $\sqrt{n}$ in the rows of Table 1 is suggested. The suggested weighting is a compromise between giving each row equal weight (and possibly over-emphasizing low frequency categories) and weighting each row by the frequency of acts observed (with possible over-emphasis of high frequency categories).

VIII. REPLICATION OF THE MORMON-TEXAN FINDING

We were not certain that results obtained from rural families more than a decade ago could be replicated with urban middle class families. To check this point, two revealed difference discussions between husband and wife were added to a current investigation in Chicago. The couples had already participated in nine revealed difference discussions with a 14 to 16 year old son or daughter. The revealed difference questions involved abstract behavioral norms rather than selection between reference couples and resultant discussions were longer and more task oriented. The administration technique was essentially the same as that described above; the recordings were again made in the home. The scores for 19 couples, made with two revealed differences each, when correlated by phase within category and weighted by the square root of the frequency, $r = 0.80$. Both the composite and more detailed correspondence is strong: The second sixth peak for 10, Disagrees, again occurs, and 7, Asks for Opinion, is again high in the first sixth. This latter point deserves note, for on the basis of the experience reported in this paper, the original hypothesis should have been expressed: opinion, Med-Hi-Lo instead of Lo-Hi-Med as shown in Table 1.

DISCUSSION

The most obvious consequence of the replication (with two populations) is to increase confidence in the essential correctness of the phase hypothesis. It was important to demonstrate that the hypothesis did hold for intimate two-person groups for it is sometimes believed that there is a discontinuity in group process as one goes from dyads to groups of three and larger. At the same time, work on two-member families does not answer legitimate questions one might raise concerning larger and less primary groups.

The information in the original hypothesis consisted of propositions about five classes of act (orientation, evaluation, control, positive, and negative) and an order relation for each according to thirds of the meeting--roughly 5 x 2, or 10 units of information. Using all 12 categories with 6 periods, one has approximately 12 x 5, or 60 units of information.

The six-to-one increase in units is high, so one is motivated to search for a more compressed way to state the hypothesis. Bales (1968, p. 468) has recently presented a translation of high and low rates on interaction categories into three dimensions: power (U, up; D, down), liking (P, positive; N, negative), and achievement (F, forward; B,

backward). U, D, and F are algebraically plus; D, N, and B, minus; and the three scales are orthogonal. The article in which these dimensional components was presented was concerned with role differentiation. In the present context of a concern for phases, it is therefore possible to suppress his distinctions between "originating" and "receiving" by adding these components together before we report values for "high" and "low" rates by category in Table 3. On the right hand side of Table 3, the observed values for the 6 periods in Table 1 have been recoded into two highs (H), two lows (L), and two intermediate values (0). Ties were resolved in support of the original phase theory. When the appropriate directional component is entered in lieu of an H or an L, and the entries in the column added, the phase hypothesis is restated as follows:

6ths	pow.	lik.	ach.
1st	-5	3	-6
2nd	0	-4	-2
3rd	4	-1	4
4th	1	-3	6
5th	-1	5	0
6th	1	0	-2
Sum	0	0	0

In this form one requires fewer (5 x 3, or 15) units of information and, in addition to this simplification, one clarifies certain other related matters. It is now easier to think of other sequential emphases. One can think of role differentiation, affect change and task accomplishment outside the context of full-fledged problems. It should be easier to map on Bales' dimensions therapy, teacher-pupil, or sociability interactions.

In addition, a limitation is suggested. If roles do become more differentiated, if interpersonal attraction does develop, and if the pressure of external demands are met, then why a return to zero? Should not some provision be made for the travel of the equilibrium point through time? In family interaction, when 60 minutes of interaction is added onto a number of years of common living, it does not offend one's intuitive sense to assume that the ultimate equilibrium point is close to that at which the interaction

TABLE 3

EMPIRICAL PHASE NORMS

RELATED TO BALES' DIMENSIONS[a]

Bales' Category	Directional Component If High	If Low	Values by Sixths 1	2	3	4	5	6
6	D P	U N	H	H	O	O	L	L
7	B	F	H	H	O	O	L	L
5	U_2 F_2	D_2 B_2	L	O	H	H	O	L
8	D	U	H	O	O	H	L	L
4	U P	D N	L	O	L	O	H	H
9	F	B	L	L	O	H	H	O
1	U P B_2	D N F_2	O	L	O	L	H	H
2	N B	P F	L	H	L	O	O	H
3	D P F_2	U N B_2	L	L	O	O	H	H
10	U N_2 F	D P_2 B	O	H	O	H	L	L
11	D N	U P	L	O	L	O	H	H
12	U_2 N_2 F	D_2 P_2 B	L	O	H	O	L	H

[a]Subscripts here treated as algebraic coefficients.

began. One takes the position that the family's complying with the request to reconcile revealed differences represents coping to maintain the status quo rather than substantial accomplishment. But if newly formed groups were involved, one would surely wish to represent the growth of friendship and role differentiation as a form of movement for the group as a whole. The present formulation implies the absence of differential implications for new, in contrast with, established, groups. It is deficient in that it does not provide for a moving equilibrium.

Finally, this schematic restatement in the three dimensions gives another way of describing the phase hypothesis. The "insurrection of the second sixth" appears to be preceded by status de-differentiation and loss of achievement focus in the first sixth. There is an assertion of status differences in the third sixth and substantial steps toward achievement in the third and fourth sixths. In the fifth sixth, positive affect re-emerges and in the final sixth, the group backs away from achievement as equilibrium is re-established.

A final topic, unrelated to the use of the three dimensions, relates to the concomitants of phase compliance. The phase reformulation is silent on whether compliance with it will be a concomitant of better adjustment when adjustment is measured by some alternative criterion. Bales (op.cit.) speaks of groups with low consensus who "hang" at particular points in the phase cycle. Landsberger's findings for mediation groups may possibly, but need not necessarily, extend to families. For example, it has recently been reported that families with normal children use more fractured sentences than do families with a schizophrenic child (Mishler and Waxler, 1968). This might suggest an exception. But these authors also report "normal" families acknowledge one another's communications more fully. This may mean that their phase compliance will be higher. One should probably avoid too literal a focus, for when Mishler and Waxler report that schizophrenic families have an unusually "controlled" affective tone, they thereby suggest that the "insurrection in the second sixth," revealed in this approach, may be viewed as an essential collective mechanism for releasing perturbations in affect. When so viewed, this aspect of the formulation--i.e., the essentiality of such perturbations--acquires salience for investigations of personality development as well as short-run task performance. Thus, silence on the relation between phase compliance and adjustment is a bit "sneaky;" we're not going out on the limb, but we think there is something to it.

THE FAMILY: PROGRAM GOALS AND OPERATIONAL DEFINITIONS

Dr. Thomas Gladwin

In this talk I want to emphasize one rather modest point. It is by no means new, but it is one we often are inclined to take for granted and thus overlook when we speak of the family. It is simply that people who want to do something with or about families are likely to conceive of the family in special ways which influence their perception of what the family really is. Whether the goal is intervention or research, or some combination of the two, the outsider approaches the family prepared to notice and to value some things in different ways than others might appraise them. In other words, what one person sees is at variance from what another person might see in the same family. Each is guided by his special interests and intentions; each selects and differentially evaluates particular aspects of the whole which in some ultimate objective sense comprises the family. Furthermore, such intervenors must have some power over the family or else their intervention would probably not be tolerated. The way they perceive the family, therefore, inevitably also influences the family, and an interplay is thus set up between what the family is and what the outsider thinks it is. This being a conference of stated advocacy, committed to the proposition that the "total family" is a good thing and provides a positive avenue for intervention with its members, the note of caution I am sounding is especially relevant. I am not saying that I disagree with the advocacy or disapprove of total family intervention. I urge only that we consider the implications of holding a view of this sort.

Needless to say, the family is not a new subject; ever since people began to care for other people they did so and continued to do so, in terms of the family. The feudal lord dealt with his serfs as families living on his land. Since then the emphasis on the family has been continuous with only a brief intermission during the industrial revolution. Yet how the feudal lord and everybody else who has treated with families since have perceived the family has varied a great deal over time. It varies with fashions, which change with time. Currently, the fashionable emphasis is on the small nuclear family. The perception varies also with the agency

program or the person who's looking at the family. What one person talks about is different from what somebody else says about the same family, depending on what the intervention program is, what its objectives are, whether or not it deals with only one part of the family and more or less ignores the rest. It varies with how the agency or the people stereotype the group of which the family is a part. The idea of a black family immediately conjures up certain kinds of things which are directly related to how white middle class program directors view black people in general, quite aside from how their program is to relate to the particular family. This is especially true not merely of black people, but of all ethnic minorities.

Note that these variations in the way a family is defined bear no necessary relationship to the objective nature of the family itself. Time and fashion will often change the definition of the family without the family itself changing. Similarly, different agencies deal with a single family. In this case the family remains in some degree objectively the same, yet each agency defines it in a different way. The fact, however, that these definitions and stereotypes do not always arise from the objective nature of the family does not mean that they are not important. They can breed controversy--Moyniham is a recent example. They naturally have effects, too, on the families themselves. The working definition that we have of the family sometimes becomes a self-fulfilling prophecy, and in other cases has paradoxical and unexpected effects.

A classic example of all of this is the much mooted poor black family. Stereotypically, it has been seen as disorganized, matriarchal, and destructive, especially to its mates. It has its own mythology. Its problems all started with slavery, so its origin is therefore in the past and not in the present, which is comforting to those who share a part of the blame. This sterotype has lasted for a very long time. It was first formulated in social science terms by E. Franklin Frazier, and was not seriously questioned by almost anybody, even by Malcolm X. In his autobiography, although he was very hard on people who put down the black man, Malcolm X has good things to say about Frazier. It wasn't until Moynihan came out with his report, essentially reiterating the stereotype, that the roof fell in on him. People began to realize that we could no longer accept the old definition. As a result of a whole lot of subtle forces, we had changed our minds without realizing it. Then when Moynihan issued his report from a policy position high in government and got President Johnson to make a speech built around it, we saw at once how pervasively important the

stereotype was even though we social scientists until that time had not undertaken even to dispute it.

As an example of how different agencies view the family, compare the school and the housing authority. The school sees the family centered around its children. There are demands on the parents that they participate in certain ways for the benefit of the children. Thus, the family is a child-centered unit for the school. But for the housing authority the family is simply a number of persons organized around a family head. For one it is child-centered and for the other family-head centered with little concern who that head may be. Programs differ not only in definitions of what the family is, but also in what is good and what is bad. When I used to be in the mental health business, I remember a constant strain which existed between the mental health unit on the one hand, which felt that the healthy family had to have a male as head and so tried to build up a father or a foster-father, and on the other, AFDC for which a man in the house was an anathema. Yet in fact we mental healthers never really stood up and confronted welfare on this issue. We talked about disorganized families, and building the strength of the family, while we knew other agencies, other white, middle class agencies, part of the same establishment that we were in, were tearing the very same families apart.

We also respond differently to families in different social groups. I have already mentioned minorities. There's a difference too in the approach to families between social classes regardless of minority status. What happens when a policeman picks up a middle class person? His usual first move is to find a member of the immediate family who will take responsibility. If he picks up a kid, he asks if the kid is doing well in school. If he is and if his father shows up and says, "I'm sorry my boy did this," the kid gets a warning and goes home with his father. If it is a poor person, young or old, he will probably be booked right away. Several essentially irrevocable acts will take place before a member of the family is involved in any way.

The different social sciences also view the family in very different ways. Dr. Ornati was telling us of the gap between how the economist sees the family and how the people-lover sees the family. In spite of the fact that most families, particularly poor families, make most of their decisions on the basis of perceived short-run economic gain, we people-lovers pay practically no attention to economics. The economists, in turn, practically pay no attention to families as loving groups of people.

The examples thus far have dealt with differences in outsiders' perceptions of the family, aside from the objective

nature of the family itself. Families do, of course, differ objectively. There are all manner of variations between families. For us anthropologists this is a major stock in trade. We talk not only about forms of polygamy and monogamy and so on, but also about families of all sizes. Some extended families are so broad that a child has innumerable, almost interchangable parental figures and, in some societies, an adult has almost innumerable spouses. There are more subtle kinds of differences too. Ritchie has an interesting study of the Maori in New Zealand where older siblings bring up younger siblings, so that the cognitive man to which the child is exposed is always of the cognitive world, the culture if you will, of other children a few years older than they are. They are never exposed to any realities of the adult world until they are in adolescence. In the last stage they are finally old enough to be dealt with by adults, and only then do they hear about the adult world. These Maoris are not keeping anything from their children; it's just the structure of the family there has this objective effect.

Families change over time, as we well know, and the shrinking of the middle class family in the U.S. has as its most striking effect the ejection of the elderly family members. This is an objective difference. But it is greatly exacerbated in its effects by our attitudes toward the old. Similarly, the way the family is perceived and stereotyped is often more crucial than its objective qualities. Referring to the projects described here, the way an intervention takes place, and I would submit sometimes the way research is done, has a real effect not only on the objective characteristics of the family as they exist but also as they are perceived by the researchers. All the stereotypes about the black family undoubtedly have some measure of truth in them; yet the recent Mill Hill studies suggest, at least in rural areas, that there are many more poor black families who somehow or other manage to contrive a male authority figure than we generally credit. How widespread and how true this is, I don't know, but it is an interesting finding. This and other studies suggest that disorganization in poor families is not as severe as we are inclined to think of it, and some indices of disorganization are not as clearly indicative of real disorganization as they appear to be. Thus, on the one hand we agree that illegitimacy means different things in different social groups, but on the other hand we continue to use this as an index of disorganization when it may not be nearly as negative an indicator as its usual interpretation would suggest.

By starting from a stereotype of disorganization, we make the assumption that poor families are less coherent and

less able to help one another than is in fact true. In perpetrating this stereotype, we develop programs which in fact limit the ability of family members to help one another, and thereby perpetuate the stereotype--the self-fulfilling prophecy. Here let me come back to the police. The police pick up a lower class person, particularly a youth, and take him in. The very first thing they do is book him. When he is being arrested and the working assumption is obviously being made that he is a loner and can call on no outside support. He's an <u>individual</u> in confrontation with the police, and the police are the establishment. Society in fact tends to treat poor people as loners, whereas it is inclined to treat middle class people as members of family. One result of this, for example in Washington, D.C., where I come from, is that most black people, who comprise the majority of the poor in Washington, have police records. This is because they are picked up and booked on the slightest pretext before any of their family are informed. Thereafter, however, police records are almost never purged of names of individuals who are not subsequently convicted of any crime. This is not only individual injustice; it also results in statistics which have something to do with how both the police and the larger society view poor people. Middle class people do not generally have police records, in part at least because their families are allowed to intervene before booking if they are picked up. Once again we see a self-fulfilling prophecy. But this discrimination also immobilizes the family as an effective mechanism for helping its own members. If one of my kids gets into trouble, I am in a much better position to help him because I can be sure I will get word before anything irrevocable has been done. But if I am the head of a black family, this is probably not going to be so. The first thing I am going to hear is that he is already in and he is already booked. The same kind of a thing, of course, is true of a lot of other programs I do not need to dwell on, although welfare is an obvious example.

A different kind of problem exists in the more positive programs, those which are prepared to help people, provided however, that they conform to a middle class stereotype of the good family. For example, lots of school programs involve parental participation. Yet some parents, for a variety of reasons--they're working, perhaps, or the child doesn't have a father--may love their children, but various pressures may make it difficult or impossible for the parents to participate. Therefore, in such programs which call for parental participation, the kids whose parents show up do a lot better, are treated better, are stereotyped better, and have many advantages over those whose parents don't show up. Yet this may be only a matter of differential access, differential objective ability to participate, and nothing more. All of

them love their children equally, but one set of children are the victims of a rather subtle form of middle class discrimination.

My talk is intended only as a note of caution. I draw no dramatic conclusions from these examples. They simply emphasize that we cannot speak of the family and then take it for granted that everybody knows what we are talking about, or even take it for granted among ourselves that we are all talking about the same thing. In fact, when we refer here again and again to something we call the "total family" we are doing little more than dealing in yet another stereotype. Of course, every attempt to conceptualize a problem has a measure of imperfection to it. I am not saying that the total family concept does not lead to some ultimate good. But we have to know the limitations of the consensus with which we are working.

The other thing we have to remember is that in most research or intervention programs, the family is in a very real sense what the program makes it to be. In the people-helping fields, self-fulfilling prophecies tend to grow like weeds. The outburst over Moynihan holds a real lesson for us. Lots of us by that time should have known better, but it had not occurred to most of us to stop long enough to think, is all this we say about the poor family still true. Nobody thought of it until Moynihan articulated the stereotype. Even then, initially, it was only the black people who protested. Thereafter, we all chorused, "Sure Moynihan's wrong," whereupon everybody jumped on him. Please do not misunderstand; there are a lot of good reasons for jumping on Moynihan. He is fair game. But in this case, our "holier than thou" attitude was not entirely justified.

Let me end on the same word of caution with which I began: If we must work with stereotypes and labels like "total family" and I agree we must, let us then be intellectually very self conscious about what we're doing. Let us also not take it for granted that we are all talking about the same thing. I know very well that we are not, and you I am sure would all agree.

STRATEGIC IMPLICATIONS OF THE CULTURE OF POVERTY CONCEPT

Dr. Thomas Gladwin

I don't propose this morning to talk specifically about family studies as such. Rather, I want to put the area of family studies and studies of people operating below their potential in a larger perspective, a perspective of the relationship between behavioral science theory on the one hand, and practice and policy on the other. Both the political and to some extent theoretical underpinnings of most of the projects which have been talked about here stem from the poverty program. Even though some of them were actually developed prior to the formulation of the War on Poverty, the intellectual ferment which led to the Economic Opportunity Act also led to many of these projects. Therefore, although at the end I will try to come to some of the specifics of family studies, let me start much further back, with the War on Poverty itself.

I want to look at it not as a critique of separate programs, but rather in its relationship to the social science theory from which it was generated. We have to start with the recognition that as of now the War on Poverty, as a strategy, as a body of legislation, and as a set of agencies, has fallen very far short of the goals of which Dr. Gibbons has just reminded us. It is almost fair to say that it has failed. This failure, if it is one, follows on an enormous investment: of hope, of an even larger array of promises and unless you want to compare it with the space program or Vietnam, a fairly substantial investment of money. But still we are faced with the same enclaves on people who are hopelessly poor, who are destined never to make it. Our programs have perhaps skimmed off a few people, the best bets, but I would guess that simple population increase has kept the absolute number of hopelessly poor people just about where it was when we started. Here I am by no means talking about only black people, or only poor whites, but also Indians, Puerto Ricans, Mexicans, Appalachians, the rest--even Samoans, Hawaiians--all manner of people.

There are a lot of different ways of looking at how this program fell so short of its goals. Moynihan in his recent book places the blame on two major sources of

difficulty. One is bureaucratic failures, compounded by an effort to have the maximum feasible participation of poor people in making policy. The other is a failure of social scientists who made, in his view, the mistake of getting away from theory and research findings and attempting to formulate policy. I think it is a gross over-simplification to place on bureaucracy the major burden of blame for a program which had many other complex difficulties built into it. But within limits, I think we social scientists do have to shoulder some of the blame. I certainly do not think that it is justified to say that the difficulty was simply that social scientists tried to put theory into practice, as if this were inherently a bad thing to do. If we are not trying to find out things which will have some use in the real world, what is social science all about? However, the essential point is that, for better or worse, the War on Poverty was perhaps the first major federal program to be built explicitly on a set of theoretical social science formulations.

A central concept was that of a culture of poverty, a lower class culture. This was first articulated by Oscar Lewis, though not, of course, with a U.S. population in view. It is also inherent in the body of theory formulated by Ohlin and Cloward in Delinquency and Opportunity which formed the basis for Mobilization for Youth, the project which provided the theoretical underpinnings for all community action projects. Essentially, they posited a maladaptive life style as the cause of poverty. Hence, they stressed developing the capability of people to achieve which would assure them greater access to opportunity.

The concept of a culture, of poverty or anything else, implies a self perpetuating style of life which is transmitted from one generation to another. It has its own dynamic and sustains itself indefinitely. This is at least one way of defining what anthropologists mean when they speak of a culture. The culture of poverty as so defined is however, seen also as self-evidently maladaptive simply because poor people are essentially ineffective in our society; therefore, it is assumed the culture, although it has characteristics which are self-defeating, is nevertheless sustained by the cultural transmission process. The alleged characteristics said to be so transmitted are familiar. They include disorganized families, an inability to delay gratification, weak future time orientation, a tendency toward violence, a tendency toward sexuality, and even vaguer stereotypes of shiftlessness, laziness, and self-deprecation. It is thus a pot pourri of sociological and psychological attributes.

This concept has proven to be extraordinarily comfortable, psychologically comfortable, for what we call the middle class establishment. It's a nice clean way to define the problem and justifies a number of nasty things. It says in the first place that <u>they</u> have to change, not us. The problem resides within the poor people themselves, not with us. Thus, we are here to help them change, but we're not here to help ourselves change. Furthermore, it provides an effective basis for discrimination. Originally, we said poor people were inferior because of their genetic antecedents. It is now fashionable to deny this, and there is good reason to deny it. But we have immediately substituted something else. They are still different from us, these poor people, because they have a different culture. They were brought up in different ways, so that although nominally they are equal and their germ plasm is the same as ours, behaviorally they are different. Therefore, in effect we must continue to treat them as different kinds of people than ourselves. That is, we can continue to discriminate, only with a new rationale. Finally, many of these behaviors--the violence, lack of insight, short time perspective, and so forth--suggest people who are somehow a little less sensitive than we are, and a little bit less perceptive of the world in which they live. This means that even if we do kick them around, it doesn't hurt them as much as it would if we kicked around middle class people who are more sensitive. No wonder the "culture of poverty" is such a comfortable formulation!

I think this comfort had a great deal to do with the political acceptability of the War on Poverty as it was originally defined. The concept of the culture of poverty was never effectively countered, except by a few lonely voices, during the entire formative period that led up to the passage of the Economic Opportunity Act. Most notable among these lonely voices was Hylan Lewis, but he has never received the credit that he deserves for this. In fact, Charles Valentine has written a whole book on the subject, <u>Culture and Poverty</u>, and he scarcely mentions Hylan Lewis except in passing. I speak about Hylan Lewis with particular poignance, because I gave a paper--the best attended paper I've ever given--at the Social Welfare Forum in Minneapolis in 1961, and Hylan was a discussant for that paper. I took a strong positive position on the concept of the culture of poverty and on community development techniques, which are essentially those involved in community action projects, as a proper solution. I got a big hand, but Hylan didn't get much of a hand when he tried to shoot me down. Now I feel ashamed at having played that part, but we were all in it, all saying the same thing, all being very uncritical about culture.

Thus a set of strategies were devised to overcome the effects of this maladaptive culture of poverty. These took one of two forms: programs designed to alter the ways of acting and thinking and perceiving one's self which were said to be learned in this culture, programs like Head Start, the Job Corps and so on, which taught the individual poor people how to adapt to a different culture. The other strategy was to change numbers of people where they were, to change their total life style, and make it more adaptive--the community action projects. What is particularly important to bear in mind is that none of these programs tried to change the larger culture. We in the middle class didn't really have to give away anything but tax money. We even hired other people to do the work.

I repeat again, I don't want now to go into these programs to criticize or evaluate them on their merits, but rather point out only that these are the kind of interventions that we have been undertaking. Yet we should have been suspicious years ago--I should have been suspicious, along with a lot of other people--because if we look at the culture of poverty as it manifests itself here and there around the world, it is everywhere extraordinarily similar. This is no accident. The poor people in Mexico City whom Oscar Lewis originally described fit current descriptions of poor people in the black ghettos of the Northern cities in the United States. And as you travel around and look at Indians, Mexicans, and all the rest, you see them all doing the same things, all having the same qualities despite their grossly different cultural antecedents. Although they each started with different cultural backgrounds, they wind up now with similar kinds of behaviors, very similar kinds of structural relationships to the larger societies of which they are a part, of social distress, not just income indices, and a whole lot of other things that are part of the same complex. This is in spite of a very diverse set of alleged precipitating causes pointed to in the history of each. Thus it was slavery which broke down black culture, the Bureau of Indian Affairs which destroyed Indians, and the wetback tradition and the peonage of Mexicans which brought them down. Yet now these, and many others, are all doing the same kinds of things and sharing the same kind of self-perceptions. This should have been enough right there to have given us a clue, enough at least to suggest that what really happened was that all of these people had been forced to adapt into a common mold in their efforts to a life of poverty, of being poor. Just as we see Eskimos adapt in certain ways we are familiar with to a cold climate, or the Bedouins to a desert environment, so we find that all people in the Arctic have to share certain behaviors with Eskimos, and all people who live in deserts have to, within limits,

do similar kinds of things as Bedouins. In some way it seems that all people who are poor in Western society wind up doing certain very similar kinds of things in their efforts to survive. What we realize then is that we are not dealing in a primary sense with a culture of poverty. Rather, we are dealing with the way our larger culture handles poor people. We are seeing the effects of being socialized into the poverty system of our total culture and not a life style which is a self-perpetuating culture of its own.

Turning to intensive recent studies of poor people, one finds the same conclusion emerging, with Eliot Liebow's Tally's Corner as a good example. Yet ironically this fact was discovered many years ago through a different set of intensive studies, although its significance was not fully perceived at the time. Anthropologists working with Indians began to recognize what was called pan-Indianism; Indians were acting like stereotyped Indians instead of distinctively like Cherokees or Navahoes. They found not only a commonality of interest among the tribes, but a commonality of point of view and life style among people ranging in origin all the way from the buffalo-hunting, scalp-taking romantic Plains Indians to sedentary agricultural groups of the Southeast and the Great Lakes. Once placed on reservations, all ended up acting like "Indians." The only ones that were able to continue being what they once were were those able to enclave themselves like the Hopis, or those massive in numbers like the Navahoes. Now we realize that Indians, like the poor, have their lives programmed for them by the larger society. Valentine in his recent book, Culture and Poverty, reviews this issue in an impassioned, almost polemic vein. He insists correctly that we are not dealing with a culture, or even a subculture with its own autonomy, its own self-perpetuating dynamics. True, there are behaviors which people see around them and presumedly learn. However, as long as our society continues to treat poor people as they do, regardless of any alternative models of behavior which may be offered, the probability is that people are going to keep right on manifesting the characteristic behaviors of poverty. When we say the behaviors are not going to change in any substantial way, this is true not only of those which we can, at a certain level, say are adaptive, but also some which are blatantly maladaptive, yet which are also shared by all kinds of poor people.

Here let me quote from Eliot Liebow's Tally's Corner:*

> Such a frame of reference, I believe, can bring into clearer focus the practical points of leverage for social change in this area. We do not have to see the problem in terms of breaking into a puncture

proof circle, of trying to change values, of disrupting the lines of communication between parent and child so that the parent cannot make children in their own image, thereby transmitting their culture inexorably, ad infinitum. No doubt, each generation does provide role models for each succeeding one. Of much greater importance for the possibilities of change, however, is the fact that many similarities between the lower class Negro father and son (or mother and daughter) do not result from "cultural transmission" but from the fact that the son goes out and independently experiences the same failures, in the same areas, and for much the same reasons as his father. What appears as a dynamic, self-sustaining cultural process is, in part at least, a relatively simple piece of social machinery which turns out, in rather mechanical fashion, independently produced look-alikes. The problem is how to change the conditions which, by guaranteeing failure, cause the son to be made in the image of the father.

This is an eloquent summary of the impossible dilemma which we have mislabeled "the culture of poverty." What Eliot Liebow is calling for is a change in the way society treats poor people. Yet we know we cannot legislate attitudes; we cannot overnight make people be nice to poor people. We tried that with civil rights, and you know what happened. It calls first for admission that we are a part of the problem, in fact that we <u>are</u> the problem--as black people have been telling white people for several years. It then calls for major structural changes in society. It calls for the reallocation of money and power and resources in such a way that we middle class whites don't have all the marbles. It calls for expensive policies such as major income maintenace at a realistic level. It calls for an effective employment policy, and for turning control of the schools over to the people who are being taught, not leaving it to an educational establishment with whose intransigence almost everybody in this room has had some experience. We have no right to retain all the goodies of our society for ourselves or even keep them under our exclusive control.

Now, where does this leave us with respect to projects such as those we've been talking about in this conference? I'm not talking here only about Project Know How or any specific project. We have to recognize that in the absence of larger societal reforms all projects which are confined to the development of individual potential among people caught in the poverty sector of our culture are not in the long run going to have any dramatic or lasting effects.

I do not mean by this that we should junk all the people-helping, people-developing projects such as have been described here. What I am saying is that we have to keep them in perspective, to understand what they are doing and equally what they're not doing. We have not only to understand this ourselves as professionals and as social scientists, but we have also to communicate this understanding to other people, and not fool anybody. It is obvious, for example, that a project which costs on the order of eight, ten or twenty thousand dollars per year per child, which most of these projects do, is not an economical model for solving the overall problem. Indeed, I think a strong argument could be made for the view that if you gave the families each twenty thousand dollars a year, they would do a lot better job of developing their children (if they were assured of having it for the full twenty years while they are bringing them up) than any program described here. These programs are not prototypes, or at least direct prototypes, for economically feasible intervention programs at all. Of course, part of the cost is for research, and so on, but we are talking about matters of major scale here. If even fifty percent of twenty thousand dollars per year per child represents the research cost, you still have ten thousand dollars direct intervention money which the family could still probably better spend themselves.

However, if we learn something really new and big from these projects, that is another matter. If we really add a major increment to our knowledge of how human development works, of how the teaching process goes, this would be a very different thing. Our investment would be well worthwhile. But instead we have tended to hear again and again at this conference, as at other conferences, we can't even determine what the net impact of the intervention itself is. Perhaps we feel that the trends are going our way, and probably the intervention has something to do with it, but we're still not sure just how it works. In actuality, we have hardly learned a damn thing. All we've done is restate an objective. We may have learned a little bit of technique here and there, and we may have demonstrated something that is as valid as the .01 level of significance, but this isn't what poverty is all about. Poverty is a very big and nasty problem. To be effective, we have to find a strategy whose results are so dramatic as to be self-evident. If we are to be honest, we must admit that the programs described here are very far from this goal. Their outcomes, viewed against the magnitude of the problem, are almost trivial. Yet as long as we pursue them and pretend we are going after root causes, when really we're not, we scientists provide an excuse for deferring the action and the sacrifices which some day our society, which assuredly can afford them, is going to have to make.

THE USE OF PARTICIPANT GROUP METHODS WITH CULTURALLY DISADVANTAGED FAMILIES*

Paul Wohlford

Early in life a child learns the style of behavior which destines him for a particular kind of social, economic, and emotional existence. As primary socializing influences on the child, the other family members directly and indirectly determine the child's personality structure, his values, his aspirations, and so on. The parents in particular occupy a key role in the socialization process, mediating the demonstrated relationships between social class variables, on the one hand, and outcome variables, like mental illness, delinquency, intelligence, and social achievement on the other. The association between social class and the socialization process has been well documented, but the nature of the processes within the family that account for this association is very unclear.

What is clear is that many government programs definitely or potentially influence family processes. There are many kinds of families, rich and poor, white and black, old and young, and so on. The ideas to follow will concern primarily a selected portion of families, the culturally disadvantaged families that happen to be poor, black, and young enough to have preschool children. The intent of the pragmatic suggestions is to aid the parents to intervene in the poverty cycle that would affect their children.

There is a rapidly growing body of theory and research regarding the critical role that early experience plays in the establishment of intelligence, achievement, motivation, interests, attitudes, and personality (Piaget, 1954; White, 1959; Hunt, 1961; Kagan & Moss, 1962; Bloom, 1964). Project Head Start was initiated largely on two arguments: 1) Educational programs that wait until the child enters public school at about age six may miss the most critical period

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*The author wishes to thank John W. McDavid for many helpful comments on earlier drafts of this paper, and Herbert M. Dandes and Harris W. Stern for their suggestions.

for intervention; and 2) The quality of the child's early and enduring experience in his home environment is a critical variable in his total educational experience. The extensive Office of Education study directed by Coleman (1966) indicates that none of the school character variables involved were as strongly related to school achievement as were the home environment variables.

With very few exceptions, educational programs have not attempted to enlist the parents of children to serve as instrumental agents in the educational process.

If public schools are secondary socializing agencies, then preschool programs like Head Start may be considered primary socializing agencies. In contrast to the middle class child's situation whose familial and school environments enhance and reinforce each other, the culturally disadvantaged child's familial and school environments may be in conflict. When a middle class, preschool child enters his neighborhood nursery school or kindergarten, he probably experiences a continuation or an extension of the same kind of values and child-rearing practices as he already experienced as nurturant role models such as love-oriented disciplinary techniques; value placed upon verbalization, curiosity, and so on.

In contrast a child from a poverty background probably has experienced something quite different: a lack of nurturant role models, sometimes a lack of supervision, physical disciplinary techniques, and restrictive demands regarding neatness, cleanliness and obedience. Many culturally disadvantaged parents, in their regular child-rearing practices, may stifle curiosity and autonomy in their child. If the child participates in Head Start, the parent may not be supporting the program's daytime work-week efforts.

In short the culturally disadvantaged child's familial and preschool environments may be in conflict. The conflicting social milieus may offset one another, and thus the preschool program's effectiveness is lost. At worst the child may internalize the incompatible values from each milieu, and be more likely to become emotionally disturbed at a later time.

In order to provide the culturally disadvantaged preschool child with a home environment that is congruent with the educational approaches in the preschool situation, the parents themselves would have to undergo important changes in their attitudes and behavior. Of various methods available to achieve such changes, participant group methods seem best.

One of the advantages of the participant group approach making it especially attractive for the present purpose is the way in which it awakens in the participants alternative modes of interpersonal relating. The individual participant is frequently driven to realize that he, in his choice of values and in his behavior, has far more responsibility for his situation than he previously recognized. It is especially important for the parent to realize how he influences his child.

Participant group methods, variously called T-groups ("T" for "Training"), sensitivity training groups, encounter groups, or human relations laboratories, have been conducted with thousands of people in industry, education, health and government to realize goals of individual growth and increased group effectiveness. The usual application of these procedures has been with well-functioning individuals from the middle class; it appears to be a very effective short-term technique of changing personality. Since the T-group goals and procedures seem highly congruent with those of desirable modes of parent-child interaction, the possibility of employing T-group procedures with the parents of culturally deprived preschool children seems not only plausible, but very exciting.

RELATED RESEARCH

Familial Influences on the Child's Development

Having a key role in the socialization of their children, the parents appear to mediate the demonstrated relationships between social class variables, on the one hand, and outcome variables, like mental illness, delinquency, intelligence, and school achievement, on the other. They shape his behavior through their interactions with him by directly modeling, reinforcing, or extinguishing his coping mechanisms, a construct suggested by the ego psychologists. If a child develops coping mechanisms which articulate and investigate his environment, rather than repress, deny, and act out, he is fortunate in at least two respects: emotionally and cognitively-intellectually. Emotionally, such a child is less likely to become a delinquent as an adolescent or a psychotic as an adult. Cognitively-intellectually, the child who explores, verbalizes freely, and asks questions, is likely to become more intelligent, perform better in school, continue his education further, and achieve more vocational success.

Unfortunately, there seems to be a cleavage within the field of child development between two schools of thought; one school focuses on cognitive development while the other

school focuses on emotional development. The cleavages between these two schools run through research studies, journals, federal agencies, professional organizations, and so on. Although many experts on both sides of the fence acknowledge the existence of, and even the importance of the other view, very seldom does this rhetoric get translated into a theory or study which does in fact embrace both sides. The following review emphasizes those views which are exceptions to the cleavage, or at least permit ready translation into the other rhetoric.

There are several studies and reviews of social class and socialization (e.g., Bronfenbrenner, 1958; Miller and Swanson, 1960). As Hess and Shipman observe, the issue of whether social class is related to socialization has been well established. Rather, the issue is, how does social class influence the socialization processes. Hess and Shipman argue,

> "...The central quality involved in the effects of cultural deprevation is a lack of cognitive meaning in the mother-child communication system...The growth of cognitive processes is fostered in family control systems which offer and permit a wide range of alternative of action and thought...Such growth is constricted by systems of control which offer predetermined solutions and few alternatives for consideration and choice.
>
> "In the deprived family context this means that the nature of the control system which related parent to child restricts the number and kind of alternatives of action and thought that are opened; such constriction precludes a tendency for the child to reflect, to consider and choose among alternatives for speech and action. It develops modes for dealing with stimuli and problems which are impulsive rather than reflective, which deal with the immediate rather than the future, and which are disconnected rather than sequential." (Hess and Shipman, 1968, p. 870-871).

Bernstein (1961) distinguished between two styles of language and communication code, the <u>restricted</u> and <u>elaborated</u> codes. <u>Restricted</u> codes are stereotyped, limited, and condensed, lacking in specificity; it is the language of implicit meaning, easily understood and commonly shared. In <u>elaborated</u> codes, communication is individualized, and the message is specific to a particular situation, topic and person. Bernstein's distinction between these two types of language codes is interlaced with variables of social interaction which generate two types of family control: person-oriented control and status-oriented control. Elaborated and person-

oriented statements lend themselves to cognitive styles which involve reflection and the comparison of alternative consequences. Status-oriented statements tend to be restrictive of thought.

The Bernstein and Hess-Shipman analyses of the mother-child communication are highly relevant to both cognitive growth and emotional growth. In fact, a recent authoritative statement of communication theory (Watzlawick, Beavin, and Jackson, 1967), emphasizes the interpersonal interactions which serve as the context for the information exchange.

Shutz (1966) has proposed a three-dimensional scheme for understanding interpersonal behavior, that would seem to serve well as the theoretical basis for understanding parent-child interactions. The three dimensions are inclusion, control, and affection. In Shutz's review of the literature concerning parent-child relationships, he concluded that the diverse findings and labels applied to parent-child interaction variables could be unified and accounted for by these same three dimensions:

> From the studies reported, our three need areas as applied to parent-child relations look something like this:
>
> Inclusion is also called parent-child interaction, stimulation, and, in the extreme, indulgence. The positive end of inclusion in parent-child relations is characterized by a child-centered home, with the child constantly subject to attention, concern, and action, a high level of activity, and intense and frequent contact with both parents. The negative end is characterized by an adult-centered home where the child is left to his own devises, neglected, ignored, understimulated; interaction with parents is low even for spankings, and disapproved activities will be ignored, including neglect of jobs, masturbation, and disobedience.
>
> Control is called democracy, control, and promotion of independence. The positive end of control includes freedom to choose, decide, originate, reject, --freedom from arbitrary control in general. More specifically, the parents characteristically justify their policies, decide things democratically, readily explain (including child's questions about sex), take child on picnics, give him his own spending money, and do not interfere in his fights. The negative aspect shows the child restrained strictly within the bounds of autocratic depotism, his

obedience is demanded, suggestions are given coercively, and regulations are restrictive.

Affection is called affectionateness, approval and affectionateness, and acceptance of the child. The positive side seems to involve behavior that is affectionate, accepting, approving, encouraging, facilitating. The negative aspect shows the child blamed, discouraged, disapproved, rejected, and inhibited, as well as not receiving affection. (Shutz, 1966, p. 40-41).

Lavin (1965) reviewed research concerning the relationship between parent-child interaction variables and school achievement. It would seem that the variables identified in his review can also be accounted for by Shutz's three interpersonal dimensions. For example, the following variables were associated with lowered school achievement: lack of belief in the family's ability to have some mastery over the world (control), little warmth in relationship with fathers (affection), parents less often described as thoughtful, understanding and interested in children (inclusion). It is also possible to identify these dimensions which Fleigler (1957) found among gifted, low-achieving children: (1) a neutral indifferent view of education by parents (inclusion); (2) overanxious, over-solicitous, easy going or inconsistent parents; (3) lukewarm (affection), indifferent (inclusion) parents; and (4) lack of cooperative spirit in the family (inclusion).

It seems, therefore, that parent-child interaction significantly affects the school-related functioning of children, and that this interpersonal behavior can in large part be accounted for by the three dimensions proposed by Shutz. Discriminating the dimension of inclusion seems especially necessary in considering the culturally deprived families, as the fathers are often absent entirely. Furthermore, the amount of time that both parents have to interact with their children may be extremely limited because of employment.

Shutz described the "ideal" parent-child interaction in each of these areas:

"Inclusion: The child's need for inclusion requires a satisfactory relation regarding interaction and association with people. The parent satisfies this need by spending a satisfactory amount of time interacting with the child...In addition to the amount of playing or interacting with the child, the quality of interaction between parent and child plays a central role in inclusion behavior. In order to feel he is really being interacted with, the child

requires undivided attention and needs to feel that he is being acted toward as a unique person...The ideal relation between parent and children in the inclusion area is between the two extremes of abandoned and enmeshed. The parent interacts widely and frequently with the child and takes an active interest in his activities but also allows the child freedom to be alone, away from interaction with people...Such a relation tends to develop children who are comfortable either in the presence or absence of others, the type that has been called "social".

"Control: The child's need for control is the need for a satisfactory relation with people regarding control and power. Behavior relevant to this need centers on the making of decisions. Problems of discipline and guidance have as their focus the question of who will make decisions and in what way. The child must be taught how to make sound decisions, and he must also be allowed to make them...The ideal relation between parent and child in the control area lies between the two extremes of dominated and undirected. The parent develops the child's ability to make responsible decisions on his own, and allows him to make them; yet at the same time he makes necessary decisions for the child, for example, about crossing the street.

"Affection: The child's need for affection is the need for a satisfactory relation with people regarding affection and love...The ideal relation between parent and child in the affection area lies between the unloving and the smothering relations. The parent gives ample affection and love to the child but not so much that the child feels overwhelmed and incapable of assimilating--and perhaps reciprocating--the love." (Shutz, 1966, p. 86-89).

Another theoretical base for the present position is Tomkins' systematic personality theory. Tomkins (1962, 1963) proposes that affects, rather than drives, constitute the most important source of human motivation; Latane and Schachter (1962) and Osgood (1962) have also recognized the importance of affective states. Affective states are in part learned through the socialization process in the family, by means of cognitive coding of experience. Cognition, in turn, is influenced by affective states or moods (Beck, 1963; Wohlford, 1966). Hence, affective states and cognitions are fundamentally related aspects of personality which develop together in the primary socialization of the child.

Previously cited research on the antecedents of achievement motivation might be considered in light of the Tomkins'

formulation about the development of cognitive capacity and the affective states which Tomkins calls interest-excitement. Tomkins' delineation of a total of eight positive and negative affective states contributes a useful addition to Shutz's theory. In particular, the important parent-child interactions of aggression and depression seem to be relevant dimensions, especially for this population. Bandura and Walters' (1963) systematic approach to social learning underscores the salience of the behavioral model and the imitation process on the acquisition of such behaviors as aggression and self-control.

Intervention Programs with Parents of Culturally Disadvantaged Preschool Children

The lower socio-economic classes are, unfortunately, the least accessible people to involve in service, educational, or remedial programs of any kind (Chilman, 1965; Richmond, 1965; Christmas, 1966; Glickman, 1968). Many different kinds of programs have been tried which were specifically aimed at involving parents of lower socio-economic class children in educational or school-related projects. These projects have attained some degree of success according to the impressionistic self-evaluations reported. For instance, Gordon and Wilkerson (1966) summarized projects for mothers of culturally disadvantaged preschool children in Ypsilanti, Pennsylvania, New Haven, and Chicago. However, there was no systematic report of how a parent's program effected changes in the parents and in their relationships with their children. Nevertheless, the literature of the different kinds of parent programs for the culturally disadvantaged will be briefly summarized.

There are three major kinds of programs: individual work, large group, and small group.

Individual work with culturally disadvantaged parents, especially mothers, falls in two categories: the very old and the very new. Many of these mothers have been recipients of social welfare service and have experienced, probably negatively, an individual worker who, because of her middle class background and larger case load, established only a superficial relationship. On the other hand, Gray (1966) and her colleagues at Peabody worked intensively with individual mothers in their homes. Weikart and Lambie (1968) reported the results of the Ypsilanti home teaching project. A recent project by Giammatteo (1967) and his colleagues in Oregon employs indigenous mothers who have been trained to work individually with mothers in their homes for educational purposes.

The role of parent-centered intervention for improved academic achievement has been demonstrated by Brookover and his associates (1964, 1966). Della-Piana (1966) reviewed several studies which showed that parent training had positive effects on oral reading improvement. Levenstein and Sunley (1967) found that mothers from culturally deprived backgrounds were effective agents of change in increasing the verbal intelligence of two year old children.

Large group methods of reaching the parents are exemplified by the PTA's, and official Head Start policy (OEO, 1967), which may be described as effective and successful among the middle class, although even this may be questioned. Large group meetings could hardly be described as effective in making contact with parents who struggle in poverty. These latter people are generally alienated from middle class society, its agencies, and especially, the schools, where many of these probably had personal negative experiences. If this is true, then the generalization of the negative experience would be especially detrimental to the parents' involvement on any level, including the mere attendance at meetings in school buildings in which they are a part of a passive audience. The school buildings themselves may have aversive properties. Finally, even if culturally disadvantaged parents were persuaded to regularly attend a large group program, it is unlikely that the program could succeed in changing the relevant behavior in the parents' interaction with their children. Such behavior is among the most deeply entrenched and least susceptible to changes, especially from a relatively passive role as a member of an audience.

Ultimately, a program that does not involve its participants has to fail. It seems that it would be easier for the parents to become involved in a small group composed of <u>others in the same situation</u>, than to become involved in other kinds of programs. The experience of conducting psychotherapy with the culturally disadvantaged indicates that group sessions are more effective than individual sessions. There are various reasons that might explain this phenomenon: attitudes toward authority, social comparisons processes, following the therapist's model, differences in the communication pattern between the middle class patient-therapist combinations, and lower class, middle class patient-therapist combinations, etc. (Frank, 1961). Whatever these reasons may be, it is felt useful to exploit this possible source of gain (Christmas, 1966).

There have been some investigations of small group methods with parents on school-related variables, although not with a disadvantaged population. Gazda and Ohlsen (1966) used group counseling with parents of bright underachieving fifth graders, and behavioral changes reflected a

decrease in psychosomatic problems. Haley (1964), utilizing Adlerian group counseling with mothers of secondary school children, reported change in parental attitudes and also some change in student reactions. Stonstegard (1962) placed both fifth grade underachievers and their parents in groups, and noted an increase in the reading level of the fifth graders. However, in none of these studies did the postulation consist of disadvantaged parents and their children, or was the participant group method explicitly used.

Nechlin (1966) at the City College of New York has led a group of parents of three and four year olds in a nursery program at the Manhattanville Community Center. He met with this group of from 15 to 20 young mothers once per week on about ten occasions. Composition of the group changed from session to session, but a nucleus of at least 10 mothers who were consistent in attendance. There was eager reception on the part of the parents for child-rearing information. The participation of members of the pilot group indicates that lower class and uneducated parents in the community can be trained to teach other parents to understand child development . These indigenous "teachers" can lead regularly scheduled discussion groups of about 10 parents each for two hours each week for a period of ten or twelve weeks, with the aid of a doctoral student consultant.

In the Wohlford-Stern Project, the first part of each meeting was devoted to the discussion and practical demonstration of various things parents can do to expand their child's cognitive world, build his verbal power, and generally, to enhance his prereading skills. This project used the participant small group method which appeared to be a potentially useful technique to evaluate and, where necessary, to intervene in the possible detrimental parent-child interactions. With this method, the other group members provide the reference group and basic impetus for change.

Rationale of Using Group Process to Effect Changes in Parents

The participant small group method, which is termed T-group method ("T" for training) or human relations laboratory, is described in Bradford, Gibb and Benne (1964). The particular strength of the participant group method is that it enables the group members to focus on, and perhaps modify, their interpersonal behavior. Several aspects of the T-group methods seem especially appropriate for the purpose of working with culturally deprived parents.

1. Task Orientation. Those implementing social change recommend the use of task orientation rather than value or

emotional orientation. A cognitive skill focused on the groups may provide an acceptable rationale to the parents. Emotional learning occurs by having the consensually agreed on task to work through the emotional problems.

2. To have role models who by repeated demonstration:
   (a) Increase communication by labeling feelings, questioning, summarizing, etc.
   (b) Encourage initiative and curiosity. Leaders are businesslike in their return to task at hand, yet informal in their manners, and sympathetic listeners as the situation demands.

3. Communication and Feedback. The use of an open communication system, such as that of a T-group, is highly congruent with the aims of a cognitive skill oriented program. That is, the permissive, democratic home atmosphere that facilitates cognitive, verbal, and intellectual skills in children (Baldwin, Kelhorn, and Breese, 1945), is highly similar to the procedures of participant, small group work of the T-group variety.

4. Communication and Candor. It is necessary for the group members to be actual participants in a democratic type group process in a completely open system. That is, there should be no hidden purposes, no secret agenda, or manipulative intent on the part of the group leaders that is not shared with the participants.

Through participation in this type of group experience, parents should become aware of and modify their interpersonal behavior, moving in the direction of having "ideal" interpersonal relationships, and, in turn, creating these kinds of relationships with their children.

Parent group meetings should fit into the context of the neighborhood, parallelling the aims of the Community Action Programs, in order to:

1. To establish a solid working relationship between the parents and their center. The lower socio-economic class parents who don't provide their children with the adequate coping mechanisms are, unfortunately, the least accessible to treatment or remedial programs of any kind. Thus, an additional, very pragmatic provisional goal is necessary to attain: to establish a trusting relationship that would be stable and solid enough to work on deeply entrenched behavioral patterns.

2. To fit in the context of the other Head Start programs such as the monthly parent meetings.

3. To facilitate group cohesiveness and emotional involvement in the group by capitalizing on pre-existing relationships among neighbors.

4. To foster the maintenance of such changes as did occur in the group by assuring some form of continued contact with other group members.

Many lower class parents, in their regular child-rearing practices, may stifle curiosity and autonomy in their children. If the child participates in Head Start, the parents are undermining the program's daytime work-week efforts. It would be much more efficient to have the Head Start parent promote and extend those values and behaviors that are recognized as a part of the make-up of intelligent, achievement-oriented individuals. In contrast to the middle class child's situation, whose familial and preschool environments enhance and reinforce each other, the lower class child's familial and preschool environments may be in conflict. One way to resolve the conflict would be to adopt the lower socio-economic or working class standards in the preschool program. That would be unacceptable as a general solution, but the spirit of this alternative should be considered.

If the middle class society has something to contribute to the culturally deprived black family, then it must be recognized that the black family also has something to contribute to middle class society. Certainly, where there is a frank recognition of two different cultures and a desire to unify them, the best in each should be retained. Such an admission necessitates a policy of creating conditions for a genuine dialogue between the lower socio-economic class (not necessarily Negro) client and the preschool program.

To resolve the discrepant influences on the child from his home and preschool environments, the other alternative is to change the family. How can this change be accomplished? At least we can say that it would not be accomplished by a frontal attack on the family's values. Those implementing social change recommend the use of a <u>task-orientation</u>, rather than value-orientation, and the use of the participant small group method, as discussed above.

A necessary component of any program at this point is to assess the outcome. It would be highly desirable to assess the effectiveness of the project in a number of different ways. Ideally, there would be an extended treatment period, before-after measures, suitable control groups, etc.

a. There are several person-units to assess, including:
   (1) The child
   (2) The parent
   (3) The parent-child interaction
   (4) The parent-parent interaction

b. Also, there are several possible assessment procedures:
   (1) Testing
   (2) Interviewing, observing
   (3) Observing at home
   (4) Observing in a structured situation, like in the school
   (5) Observing at the parent meetings

Conclusions and Recommendations

1. Although the preceeding discussion has dealt primarily with a particular target population of the culturally disadvantaged, the lower socio-economic class black, the thrust of the participant group method appears to be equally appropriate for most other target populations of the culturally disadvantaged. Group methods are increasing in popularity with middle class populations.

2. Participant group methods may be a useful adjunct to a variety of programs, such as adult education, job training, etc., as well as a preschool child development program.

3. A basic modification of the present approach may be necessary in the case of families with school age children. To deal effectively in this situation may demand a family group arrangement, as in the Wiltwyck project (Minuchin, Montalvo, Guerney, Rosman, and Schumer, 1967). In this approach, one or more families, including all the children, would participate.

4. The diagnostic classification value of the participant small group method has been clearly demonstrated. Group experiences may involve the participants in a meaningful way for providing a relevant sample of behavior.

5. The group's potential for treatment, or change of behavior, is evident, but has not been demonstrated conclusively. Relative of other groups, such a group would have to be:
   (a) Longer,
   (b) More intensive, and,
   (c) More closely tailored to fit individual needs--perhaps to have additional work in natural neighborhood groupings of two or three.

6. Research is needed on the nature of the attitudes, motives, and behavior of parents who send children to preschool programs.

7. Research is needed on the nature of relationships between the parental behavior and the preschool child's behavior.

8. Future parent groups should take into account the child-group phenomenon and recommend appropriate methods.

9. Whatever objectives are to be pursued for the parents should be stated in behavioral terms and embedded in highly specific, manipulative work--learning experiences. Thus, perhaps involving the parents in actually making materials, while discussing their use and their children, might be a good technique to follow.

10. A great deal of sensitivity must be used in picking topics and making assignments. The mothers are, as our experience so far indicates, quite insecure and anxious. All things need to be made very explicit.

11. The parents who have participated in previous projects probably were not the most representative parents, but were probably a sampling from among the relatively well intact parents. In terms of the probabilities of success of previous efforts to work with the culturally disadvantaged, the selectivity of parents has been understandable.

To get the voluntary participation of those families who are judged hardest to involve might well require the use of regular pay for participation. If this suggestion sounds frivolous or wasteful, it should be remembered that these hard core poverty families are precisely those who need help the most. Even more in need of help--perhaps to be designated as the "hardest core families", are those within the poverty designation who are so unorganized and disrupted as not to be able to avail themselves of any organized program.

12. In future projects, it may be helpful to apply a strictly geographical limitation to assess the role of neighborhood ties. On the one hand, neighborhood relationships would seem conducive to the program goals, and on the other hand, may accentuate the alienation and anomie that some have described as existing in the Negro ghettos--the fear, suspicion and jealousy that exists among members of neighborhood groupings. Yet it is just this kind of malevolent influence that the groups may prove effective in volunteering to provide, consequently, a more stable social milieu for the children. Future projects may wish to

maximize this kind of influence by recruiting the parents from a four or nine square block area, including all strata of parents, married or not, educated or not, paid or not, and so on.

13. Future projects would be well advised to assess more thoroughly several of the above factors, especially the parents' I.Q.'s and reading level, and also the parent-child interaction in limited structured situations, such as reading and asking questions.

14. The group of parents in previous projects probably represent the top third of this center in articulateness, dedication to their children, and cooperation. This implies several things. First of all, the parents at the other extreme of the population must desperately need help and need to be reached. Secondly, it implies that any project should start out with perhaps twenty or more parents, if it is desired to keep attendance up to twelve or so. There are simply too many reality factors that prevent these parents from coming every single week.

Selection of the Specific Methods

Besides activities involving the parents alone, there are a number of other options that may be used, either by themselves or together with other methods.

One intriguing possibility is the concept of the visiting teacher or teacher's aide to visit the home to demonstrate good techniques of child rearing, much as the public health nurse does on matters of sanitation and nutrition. The demonstration of the proper techniques in the familiar environment probably does much to facilitate the implementation of the skills, as research indicates that learning transfers more easily when the environment is constant.

While the home visit method may present many difficulties, future projects should consider two more feasible variants. First, the home visit by a professional may be done in conjunction with other training procedures. For instance, in a group-training program, it would be desirable to have the group leader visit each mother in her home, both prior to the group meetings to establish rapport, and perhaps, two or three times toward the end of the group meetings to observe how the mother is implementing the recommended procedures and to answer questions on any particular problems that may arise. Secondly, perhaps other culturally deprived mothers may be trained to assist in the home interviewing, and perhaps even to lead the groups. In a recent analogous

situation, the utilization of subprofessional personnel in mental health appears to be successful if there is careful screening of the candidates and good supervision.

A second major method is focused on the transactions between parents and children; both the natural behavior observed in the home, and the elicited behavior observed in structured work.

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